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ABSTRACT

This report from the Consortium for Longitudinal Studies summarizes findings of current analyses of the educational outcomes of low-income, primarily black children who did or did not participate in experimental early education programs in the 1960s. Data from 11 research projects provided information on approximately 3,000 low-income children. The data, collected over a 20-year period, included family background data; IQ test scores; school record data; youth interview data concerning achievement orientation and self-evaluation; parent interview data on parental aspirations for and satisfaction with their children; and post-high school education, training and job experiences. Preliminary analyses of data from three projects were performed on a variety of educational achievement variables. The major question addressed by the analyses was: Are there long-term effects of early childhood programs? Results indicate that early education leads to reductions in the rate of assignment to special education classes and reduction in rate of grade retention. Also presented in this report are the activities of the Consortium staff during fiscal year 1980 in the areas of dissemination of findings, data collection and analysis, as well as a summary of the work accomplished through 1979. A theoretical model of the schooling process is presented, with detailed descriptions of the system of equations and variables used in the study. Attachments A through F present related materials. Drafts of papers on Head Start program evaluation, research activities and findings of the Consortium, methods of evaluating program outcomes, and differential long-term effectiveness of early education are also included. (Author/RH)

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PERSISTENCE OF PRESCHOOL EFFECTS:

Status, Stress and Coping Skills

Year 2 Report

Grant No. 90-C-1311(03)

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Abstract

The Consortium for Longitudinal Studies is a collaborative effort of eleven research groups investigating the long-term effects of early education programs. This report summarizes findings of current analyses on the educational outcomes of low-income primarily black children who did or did not participate in experimental early education programs in the 1960's.

The central data bank contains information on approximately 3,000 low-income children; this information was collected over a 20-year period and includes family background data, IQ test scores collected at three times, school record data from elementary through high school, youth interview data concerning achievement orientation and self-evaluation, parent interview data on parental aspirations for and satisfaction with their children, and post-high school education, training and job experiences.

This report presents the activities of the Consortium staff during FY1980 in the areas of dissemination of findings, data collection and data analyses, as well as a summary of the work accomplished through 1979. A theoretical model is presented, with detailed descriptions of the system of equations and variables.

Preliminary analyses were performed on a variety of educational achievement variables. Detailed discussions are provided for the conceptualization and operationalization of each variable. The major question addressed by these analyses was: Are there long-term effects of early childhood programs? One set of analyses investigated program/control differences in educational attainment. Regression analyses

indicated a trend such that youths who attended early education programs compared to youths who did not, were more likely to complete high school. An investigation of intervening (or confounding) variables showed that females were more likely to graduate from high school than were males, and youths were more likely to complete high school if their mothers had completed high school.

In an extension of our previous investigation of assignment to special education classes and retention in grade, year-by-year data have now been collected. Descriptive statistics are reported on three operationalized variables of this time series data. Generally, these preliminary statistics indicate that early education results in reductions in rate of assignment to special education classes and reductions in rate of grade retention. The impact of early education appears after several grades rather than immediately. When the outcome measure was nonstandard progression, a broad composite variable, early education resulted in reduced rates of assignment, in seven of eight projects.

Preliminary analyses of two projects on track assignment in high school (college preparatory vs. regular vs. vocational) found no differences between youths who had participated in early education programs and youths who had not. Analyses of four projects on course mark data in grades 4, 6 and 10 found no strong relationship between early education experience and course marks.

The Karnes' Project has constituted an ex post facto control group, which has made their data fully utilizable in the 1980 analyses. A justification of development of the ex post facto control group is presented and comparability of the program and control groups is assessed; analyses indicate they are comparable on background variables. Updated attrition analyses for subjects included in data analyses reported here indicate that arrtition was essentially random and thus would not bias results. Less than 4% of the subjects who were located refused to be interviewed.

Work plans for FY1981 include further analyses on: a) the effect of early education on additional outcomes, both educational and labor market; b) relationships between early and later outcomes, focussing on year-by-year data; and c) the effect of early education when family background variables are incorporated. In addition, a volume on this research will be produced in collaboration with the Consortium members.

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INTRODUCTION

Overview

This report is based on a continuing investigation of the long-term effects of early educational experiences on young people from low-income families. The research of the Consortium for Longitudinal Studies is an unusual collaboration between independent investigators (E. Kuno Beller, Cynthia and Mark Deutsch, Ira Gordon, Susan Gray, Merle Karnes, Robert Jester, Phyllis Levenstein, Francis Palmer, David Weikart, Myron Woolman) and an independent analytic team (supervised by Irving Lazar and Richard Darlington).

The subjects in this study were all from low-income primarily black families who in the 1960's had either participated in research or demonstration early education programs or served as controls. Two collaborative follow-ups were conducted by the Consortium investigators, one in 1976-77 when subjects were 9-18 years old and another in 1979-80 when subjects were 13-22 years old.

Over the past five years, our multisample meta-analysis effort has primarily addressed two general questions: Were there long-term effects of early childhood programs' and were programs more effective for some subgroups of the low-income population than others. In order to answer these questions, data on age-appropriate outcome measures were collected at each follow-up. The outcome measures we used to assess the impacts of early education were, where possible, designed to indicate an individual's adaptation to her/his own social psychological environment. These "ecologically valid" outcome measures were designed to be meaningfully related to the context of an individual's own setting such as school or the world of work.

As the subjects involved in many of the Consortium programs reached young adulthood, it became possible to investigate the association of early education with such later outcome variables as graduation from high school, college attendance, successful employment, type of employment and the achievement of a self-supporting life style. Earlier analyses (Consortium, 1977, 1978, in press) had revealed a positive early education effect well beyond the termination of treatment in the lower rates of retention in grade and placement of program participants in special education in comparison to their controls. These findings lent plausibility to the hypothesis that an early education effect might be found in variables whose measurement was even more removed in time from termination of treatment, such as educational attainment and labor market success. Towards the end of hypothesis testing, the Consortium staff conceptualized a model for the association of early education and certain background and intervening variables with schooling and occupational outcomes.

Efforts in fiscal year 1980 have been concentrated on conceptualization of analyses to address our research questions, wide dissemination of findings from the Consortium meta-analyses completed in 1978-79, continuation of follow-up data collection with 1979-80 instruments, computer file management of our large data base, liaison with other Consortium members, and preliminary descriptive analyses of data collected in the 1979-80 follow-up. This Second Annual Interim Report - FY 1980 provides a description of work completed October 1, 1979 to October 31, 1980, obstacles and problems encountered, work in progress, and plans for the future.

Background of Study

Early education programs -- epitomized by the nationwide Project Head Start -- were initiated on a large scale in the 1960's as a means of decreasing the gap in school performance between middle-class and low-income children. Based on theories of the crucial nature of early experience for the course of later intellectual development (Hunt, 1961; Bloom, 1964), early childhood education programs attempted to provide children from low-income families with stimulating experiences designed to foster their cognitive and social development. Early optimism about the outcomes of such intervention was dampened by the Ohio/Westinghouse report on Project Head Start. It concluded that preschool did not have a lasting effect on children's intellectual development. Similarly, research on other early intervention programs reported that preschool participants' gains on intellegence test scores disappeared after a grade or two of public school (Bereiter, 1972; Bronfenbrenner, 1974).

The earlier evaluations of early childhood education programs were open to question on methodological grounds such as the noncomparability of the control or comparison groups, the weakness of the measurement instruments, and the failure to follow children further into their school careers (see, e.g., Barnow & Cain, 1977; Brown, 1978; Cook & Campbell, 1979; Datta, 1976; Horowitz & Paden, 1973; Magidson, 1977). In addition, reviewers who attempted to summarize the results of many different preschool research projects were hampered by a lack of comparable measures across projects (e.g., Ambron, 1977).

First Consortium Follow-up 1976-78

The Consortium for Longitudinal Studies was formed in 1975 in response to a concern that the previous evaluations of early education programs were, for the most part, methodologically inadequate. Members reasoned that well-designed longitudinal studies were important for reaching conclusions that would inform the policy-making process. Collectively, the infant and preschool programs represented in the Consortium overcame many of the shortcomings of earlier studies. The subjects were old enough in 1975 (8-17 years) to investigate the long-term effects of programs. There were treatment and control groups available. There was detailed information on the subjects' intellectual abilities and family backgrounds both before and after program participation. Collaborating on the follow-up helped insure that common outcome measures would be available for evaluating program impact.

The original Consortium early education programs were located at 11 urban and rural sites in the Northeast, Southeast, and Midwest. Their delivery systems and curricula varied considerably. Some programs concentrated on home visits to teach mothers how to be more effective teachers of their infants and toddlers; other programs taught 3- or 4-year olds in a group setting; others included both center and home visit components. Curricula in the centers included programs based on the Bank Street child development model, and others. Characteristics of the Consortium early education programs are shown in Table 1. For complete descriptions of individual Consortium projects, see selected bibliography listed in Section VI or contact investigators directly at addresses shown inside front cover.

For the 1976 follow-up, five instruments were selected by Consortium members These instruments were: The 1976 Consortium Parent and Youth Interviews, the Wechsler Intelligence Scales (Revised), achievement test scores, and school career information recorded by field workers on Consortium School Record Forms. The Parent Interview included information on educational attainment and occupation of household members, parental aspirations for sample children, and retrospective parental evaluation of preschool programs. The Youth Interview included data on school attitudes, jobs, and spare time activities.

The investigators sent 1976-77 follow-up data to Cornell for a total of 2008 of their subjects (see Table 2). The median recovery rate for pooled analyses was 74%. Virtually all attrition was due to inability to find subjects; less than three percent of the recovered subjects refused to participate. Detailed attrition analyses indicated that attrition was essentially random, introducing no noticeable biases into the data analyses.

Summary of results 1976-78. Analyses of data collected in the 1976 follow-up showed that early education programs for children from low-income families had positive long-lasting effects. These results have been reported in detail (Consortium, 1977; Consortium, 1978; Consortium, in press; Darlington, et al., 1980). To summarize briefly, we found long-lasting effects in the following areas:

1. School competence. Children who attended early education programs were significantly more likely to meet their school's basic requirements for adequate academic performance. Controlling for family background factors and initial ability, program graduates were significantly less likely to be assigned to special education classes and less likely to be retained in grade than were controls. The effect apparently operated for all the children regardless of sex, ethnic background, initial ability level, or early family background factors.

2. Developed abilities. Children who attended early education programs surpassed their controls on the Stanford-Binet intelligence test for several years after the program had ended. There was no evidence that early education programs differentially helped certain subgroups of children (differing on sex, initial ability, and family background) on IQ test scores. There was some indication that program graduates performed better on achievement tests than did controls.

3. Children's attitudes and values. In 1976, children who had attended early education programs were significantly more likely than were controls to give achievement-related reasons, such as school or work accomplishments for being proud of themselves. Older program graduates also rated their school performance significantly better than did controls.

4. Impact on family. Early education also affected maternal attitudes toward school and achievement, including satisfaction with school performance and vocational aspirations relative to those of the child.

Table 1

Characteristics of Early Education Programs and Ages of Subjects for Each Data Set

Principal Investigator	Project Name and Location	Delivery System	Birth Year	Age at Entry	Program Length	Years of Program
Beller [a]	The Philadelphia Project; Philadelphia, PA	Center	1959	4 yrs.	1 yr.	1963-64
Deutsch	Institute for Developmental Studies; New York, NY	Center	1958-1966	4 yrs.	5 yrs.	1963-71 (8 waves)
Gordon/Jester	Parent Education Program; North Central Florida	Home	1966-1967	3 - 24 mos.	1 - 3 yrs.	1966-70 (3 waves)
Gray	Early Training Project; Tennessee	Center/Home	1958	3.8 or 4.8 yrs.	14 or 26 mos.	1962-65
Karnes	Curriculum Comparison Study; Champaign-Urbana, IL	Center	1961-1963	4 yrs.	1 - 2 yrs.	1965-67 (2 waves)
Levenstein	Mother-Child Home Program; Long Island, New York	Home	1964-1968	2 or 3 yrs.	1 - 2 yrs.	1967-72 (5 waves)

Table 1 (Cont.)

Principal Investigator	Project Name and Location	Delivery System	Birth Year	Age at Entry	Program Length	Years of Program
Miller	Experimental Variation of Head Start Curricula; Louisville, JY	Center & Center/ Home	1964	4 yrs.	1 yr.	1968-69
Palmer	Harlem Training Project; New York, NY	Center	1964	2 or 3 yrs.	1 or 2 yrs.	1966-68
Weikart	Perry Preschool Project; Ypsilanti, MI	Center Home	1958-1962	3 or 4 yrs.	1 or 2 yrs.	1962-67 (5 waves)
Woolman	Micro-Social Learning System; Vineland NJ	Center	1966-1968	4 to 5 yrs.	1 to 4 yrs.	1969-73
Zigler	New Haven Follow-Through Study; New Haven, CT	Center	1962-1964	5 yrs.	4 yrs.	1967-71 (2 waves)

[a] Beller used a different designation of program group from the one shown here. In his own study, the nursery group (group 1) received two years of preschool while his second group received one year of preschool, i.e., kindergarten.

Table 2

Number of Cases with Original Data and 1976 Follow-Up Data by Instrument

Project	Original Data	1976 Follow-up Sample[a]	Parent Interview	Youth Interview	School Record Form	Ach't. Tests	IQ Score[b]
Beller	170	126	112	116	119	115	--
Deutsch	1097	97	73	88	54	44	88
Gordon	309	107	107	106	107	103	90
Gray	92	77	72	69	75	72	72
Karnes	228	168	165	156		143	112
Levenstein	250	186	98	75	154	115	75
Miller	271	141	141	141	139	134	141
Palmer	309	228	143	144	223	197	132
Weikart	123	123	106	104	123	96	110
Woolman	611	611	54	97	611	349	95
Zigler	156	144	---	141[c]	144	142	143
Totals	3593	2008	1071	1237	1900	1510	1058

[a] Follow-up sample *n* is the number of cases with data on at least one instrument.

[b] Scores include WISC-R scores for Gordon, Gray, Karnes, Miller, Palmer, Woolman; WISC scores for Levenstein, Weikart; WAIS scores for Deutsch; and PPVT scores for Zigler.

[c] Zigler interview only (some items comparable to Consortium Interview).

Second Consortium Follow-up 1979-81

Beginning in fiscal year 1979, a 3-year grant was awarded to continue research into the long-term effects of early education programs on children from low-income families. Efforts were to be directed toward wide dissemination of findings from previous Consortium analyses, supervision of the 1979-80 follow-up (design of instruments, coordination of data collection at field sites, data cleaning, addition of 1979-80 follow-up data to Consortium computer file) and extension of analyses of the effects of early education.

The research focus for the 3-year grant period is on (a) determining whether early education is related to other later outcomes such as employment, high school completion, education beyond high school, family formation, attitudes toward work, aspirations and expectations, and self-concept; (b) exploring the role of ecological factors, both before participation in early education programs and later in the subjects' lives (1976-77), including how they predict children's later school outcomes and how they interact with early education; (c) exploring the roles of critical events and support systems in the developmental histories of individuals; and (d) examing differential effectiveness of early education for children with various characteristics. Our ultimate goal is to construct analyses that relate the antecedents to the consequences of school success and failure, incorporating the influence of early education program participation, family background and process measures, and measures of subjects' intellectual ability and social psychological attributes.

In the following sections of this report, we present the activities and accomplishments in FY 1980 in the areas of dissemination of findings, data collection, and data analysis.

DISSEMINATION OF FINDINGS (FY1980)

Dissemination of Consortium research findings is a major continuing function for Ithaca personnel. In the past fiscal year we have prepared publications for the academic and policy-making community, presented papers at professional meetings, and made numerous presentations to child care workers, educators, and state and local government personnel. Consortium work also received extensive news media coverage allowing us to reach large and diverse audiences. Dissemination activities in these areas are summarized below.

Papers and Publications of Ithaca Group (FY 1980)

The following publications were prepared in FY 1980:

*-Preschool programs and later school conpetence of children from low-income families. Science, April 1980, 208, 202-204.

*-A follow-up of participants in early childhood programs. In S.A. Mednick & M. Harway (Eds.), Longitudinal Research in the United States, in press.

*-Lasting effects of early education: The Consortium for Longitudinal Studies. Monographs of the Society for Research in Child Development, in press.

*-Studying program outcomes. In B. Spodek (Ed.), Handbook of research on early childhood education. Free Press/Macmillan, in press.

*-Parents as partners: Preschool learning and school achievement, Educational Leadership, in press.

As indicated in the above listing, the article for Science was prepared and published within this fiscal year. We have received approximately sixty

*See Attachment E.

requests for reprints of the Science article and correspondence from many parts of the world, indeed, from every continent.

Reprints were distributed to Consortium members for their own dissemination needs.

The editors of the *Monographs of the Society for Research in Child Development* invited us to adapt the 1978 Consortium Report, *Lasting Effects after Preschool*, to the style of a scientific publication. In February, 1980, we sent the first draft of the revised manuscript to the SRCD Monograph editor and to all Consortium members for their feedback. Over the succeeding months, individual investigators prepared extensively detailed and helpful suggestions. Following discussions at the Consortium meeting in May, a preface to the *SRCD Monograph* was prepared by a subcommittee of Consortium members. The second draft incorporating the comments, revisions and new preface was sent to Consortium members in September for approval. Revisions were again incorporated and the revised draft was sent to the editor; she will send the manuscript, *Lasting Effects of Early Education*, to reviewers.

The publications listed below were prepared prior to FY1980. They are currently in press or were published in FY1980.

- Invest early for later dividends. *Compact Magazine*, Fall 1979.
- *Persistence of preschool effects: Status, stress, and coping skills*. Year 1 Report, Grant No. 90-C-1311 (03), October 1979.
- Policy implications of preschool intervention research. In H.C. Haywood (Ed), *Proceedings of NICHD Conference on Retarded Development*, July 1978, in press.
- Social research and social policy. In Haskins & Gallagher (Eds.), *Family Policy*. Chapel Hill: University of North Carolina Press, in press.
- Some social and motivational effects of preschool intervention. In D. McClelland (Ed.), *Education for values*. New York: Irvington, in press.

-Persistence of preschool effects. In Selected proceedings of international workshop on the at-risk infant, Tel Aviv, July 1979, Netherlands: Excerpta Medica, 1980.

We continued to receive numerous inquiries about the full technical report, Lasting Effects After Preschool, DHEW Publication No. (OHDS) 79-30178, 1978, and the non-technical summary. These requests were referred to the Government Printing Office. In spring 1980, an additional 4,000 copies of the Summary report were printed in Washington.

Presentations of Ithaca Group to Professional Societies (FY1980)

-Lasting effects after preschool. Presented at the meeting of the International Congress on Early Childhood Education, Tel Aviv, January 1980. (Richard B. Darlington)

-Differential effectiveness of early education: Testing for long-term interactive effects. Presented at the annual meeting of the American Educational Research Association, Boston, April 1980. (Paper prepared by Jacqueline Royce, Reuben Snipper, Harry Marray, Judith Nelson.)

Presentations to Educators, State Child Development and Human Service Groups, and Policy-Makers (FY1980)

Dr. Irving Lazar has maintained a heavy speaking schedule in the past year. A partial listing of his presentations of Consortium findings since November 1, 1979 are summarized below.

-University of San Francisco, November 1979.

-ACYF -- International Year of the Child group, November 1979.

-State Department of Education, Hartford, Connecticut, November 1979.

-State Department of Education, Montogomery, Alabama, November 1979.

- State Department of Education, Oklahoma City, Oklahoma, November 1979.
- Minnesota Council of Quality Education and State Department of Education, St. Paul, Minnesota, November 1979.
- State Department of Education, Harrisburg, Pennsylvania, December 1979.
- Massachusetts Council on Children and Youth, December 1979.
- Legislative Commission on Education, Chicago, Illinois, December 1979.
- California Association for the Education of Young Children, Fresno, California, March 1980.
- Stanford University, March 1980.
- Conference on Parenting, Chapel Hill, North Carolina, March 1980.
- Federal Inter-Agency Panel on Early Childhood, March 1980.
- Cedar Sinai Hospital, Los Angeles, California, March 1980.
- Nevada Association for Handicapped Children, March 1980.
- University of California - Los Angeles, April 1980.
- Research Conference, White House Conference on Family, Washington, DC, April 1980.
- Busch Seminar, U.C.L.A., Los Angeles, California, April 1980.
- California Association for Exceptional Children, Los Angeles, April 1980.
- State Department of Education, Charleston, West Virginia, May 1980.
- Institute on Poverty, University of Wisconsin, Madison, May 1980.
- Indo-U.S. Commission on Effect of Early Deprivation, New Delhi, India, May 1980.
- National Head Start Directors' Conference, Miami, Florida, May 1980.
- Perspectives on Head Start Workshop, New York, New York, July 1980.

- Southwest Regional Head Start Conference, Dallas, Texas, August 1980.
- New York State Assembly Committee on Child Care, Albany, September, 1980.
- Minnesota State Council on Quality Education, Legislative Symposium, Minneapolis, September 1980.
- New York State Departments of Education and Mental Health, Seminar on Early Intervention, Albany, October 1980.

Richard B. Darlington addressed the Montessori Society in June 1980 in New York City. Jacqueline Royce presented a workshop at meetings of the New York State Association for the Education of Young Children in Syracuse in May 1980.

Citations and News Media Coverage (FY 1980)

Consortium work has been reported in several important texts (e.g., Zigler & Valentine, 1979; Bronfenbrenner, 1979; Goodwin & Driscoll, 1980) and has received extensive editorial comments and news coverage including articles in the New York Times, The Washington Post, and Newsweek. A selected listing is given below.

- A Head Start in the Nursery, Psychology Today, September 1979 (by Maya Pines).

*-A High Grade for Head Start. Newsweek, October 8, 1979.

*-A Triumph of Social Engineering. New York Times, November 3, 1979, (Editorial).

*-Lasting Gains Are Found From Preschool Start. The Washington Post, November 30, 1979.

- Head Start: Long-Term Success Reported. International Herald Tribune, December 1-2, 1979. (WP)

* See Attachment E.

*-Federal Study Finds Poor Children Helped by Preschool Classes. New York Times, December 1, 1979 (AP)

- Preschool Classes Do Give A Head Start, Study Finds. Philadelphia Inquirer, December 3, 1979. (AP)

*-Head Start: A Success Story After All, New York Times, December 9, 1979, (Albert Shanker - "Where We Stand" column).

*-Head Start Efforts Prove Their Value. New York Times, January 6, 1980, (Winter Survey of Education).

- First of All Confidence, Project Head Start, I. Zigler & J. Valentine (Eds.), New York Times Book Review, January 6, 1980, (reviewed by Diane Ravitch).

- Cornell University Says Head Starters Fare Well. Human Development News, January 1980.

- Lasting Effects After Preschool: A Case Study in How To Evaluate Education Programs. A monthly guide to programs that work, March 1980, Washington, DC: Capitol Publication.

*-Moving Head Start Ahead, The Washington Post, April 30, 1980 (Editorial).

*See Attachment E.

COLLECTION OF 1979-80 FOLLOW-UP DATA

The purpose of the 1979-80 follow-up effort was to continue the study of the long-term effects of early education in the lives of the Consortium sample. The older Consortium subjects, in their early twenties in 1979-80, were old enough to have completed (or dropped out of) high school, to start families, and to be employed or looking for work. The present grant provided an opportunity to ascertain the educational and vocational status of our sample and the particular stresses and coping strategies used in their social context. The grant provided funds for administering a common achievement test to the Consortium subjects who were still in school (14 to 16 years old in 1970). We have shown that preschool participants had lower rates of assignment to special classes and grade retention, but we did not know when these school failures occurred. The FY 1979-80 funds enabled us to collect details on the school careers of Consortium subjects so that developmental issues could be addressed.

The Ithaca staff were responsible for designing and pretesting the data collection instruments and, in FY1979 and FY1980, for coordination and supervision of data collection at field sites and preparation of the raw data for keypunching.

The 1979-80 data collection effort involved (a) collecting data from school records; (b) interviewing the subjects who were old enough to be high school graduates in 1979-80; (c) for the younger subjects, administering a common achievement test, (d) reporting on school district policies regarding dropout prevention and retention/special education placement; and (e) compiling case studies.

Instruments

The 1979 School Data Form and the 1979 Youth Interview are instruments designed by the Ithaca staff in consultation with other investigators and staffs. Instruments were pretested in Ithaca, New York and in Murfreesboro, Tennessee. The 1979 School Data Form consists of three sections: Part A - information about the student's school career to date, including progress through the grades, absences, suspensions and expulsions, honors, and referrals; and Part B - detailed information on underachievers, including diagnoses and reasons for recommending special services, critical home and school events, and support systems. A suggested interview of school administrators regarding school district policies was also designed (Part C).

The 1979 Youth Interview focuses on determining subject's status in the areas of education, employment, and family formation. Additional items include job satisfaction, attitudes toward work, educational and vocational aspirations and expectations, sources of income, desired and expected family size, stresses and supports during childhood, self-evaluation, self-esteem and life priorities. (See Attachment D for copies of these instruments.)

1980 Sample and Status of Data Collection

Projects whose subjects were old enough to have graduated from high school in 1979 (Beller, Deutsch, Gray) collected follow-up data, primarily at the end of FY1979. These subjects were interviewed and information about their school performances on a year-by-year basis was collected from their school records. Weikart also collected follow-up data in FY1979 and FY1980. In FY1980, data collection was scheduled for projects with subjects aged 14-18 years (Gordon/Jester, Karnes, Levenstein, Miller, and Palmer). School record information was collected, subjects were interviewed, and some achievement tests were administered or scores collected from school records.

Data received at the Ithaca office as of October 1, 1980 are shown in Table 3. An account of the data collection for each project site follows along with any data collection problems or obstacles encountered. An important addition to our data set is a control group in the Karnes project. Development of this ex-post facto group is discussed below and, in greater detail, in Attachment B.

Beller. The 1976 follow-up had included 126 subjects. In the fall of 1978, these subjects were 20 years old. As shown in Table 3, the 1979 School Data Form (Part A) was obtained for 121 of those 126 subjects. Part B forms were completed in Ithaca from narratives sent by the Beller staff. The Beller staff compiled a report on Philadelphia school ststem policies for relevant years (Part C of School Data Form). The 1979 Consortium Youth Interview was administered by telephone to 86 subjects. The interview typically took 30 minutes and was well received by interviewers and respondents. Because of age and mobility, retrieval of additional subjects for the interview has proved difficult. Some subjects have no known phone number or address, and others have moved out of the area (presumably temporarily). Only four subjects were not interested in being interviewed.

The illegal activities survey was mailed to respondents along with the subject's payment for the telephone interview. The subjects then mailed their anonymous responses to the illegal activities survey directly to the Ithaca office. Approximately 50% of the subjects have returned the questionnaire.

Deutsch. The subjects in this project are representative of the students in the Consortium early education program; that is, they fall near the middle of the samples of mean pre-program IQ scores of children, mean educational attain-

Table 3

1979-80 Follow-up Data Received by Project as of October 1, 1980

	School Data Form				# Cases
Project[a]	Part A	Part B[b]	Interview	Ach't Test	Any Data
Beller[c]	121	13(16)	86	X	122
Deutsch	23	--	24	X	24
Gordon/Jester	5	4(5)	22	24	22
Gray[c]	86	--	83	X	86
Karnes[c]	195	120(201)	120	X	207
Levenstein	50	7(7)	X	X	50
Miller	120	19(27)	156	91	172
Palmer	219	--	X	185[d]	219
Weikart[c]	53	9(9)	X	X	53
Totals	872	172(265)	491	300	955

Note. X indicates that instrument was not scheduled for data collection at present.

[a] Data received in alternate form include: Beller - underachiever information included in Part A, 42 Beller subjects have returned anonymous illegal activities questionnaires; Deutsch - school data obtained in interview; 6 case studies derived from interviews; Weikart - related data sent for 41 Curriculum Demonstration subjects.

[b] Number in parentheses indicates number of Part B ("Underachiever") forms received. Some cases had psychological evaluations for more than one year.

[c] Beller and Karnes staffs completed interviews with school district personnel and wrote reports on the policies regarding dropout prevention programs and retention/special education placements (School Data Form - Part C). Gray and Weikart sent decriptions of school policies in alternative form.

[d] Manhattan 8th grade achievement test scores sent in 1979. In addition, 9th grade achievement test scores were sent for 162 cases in 1980.

[e] Not including achievement test data.

ment of mothers, and mean SES. This makes the Deutsch's sample particularly appropriate for in-depth study. Following a case study approach, the Deutsches used prior data and also collected new data on treatment subjects who had been contacted in 1976-77 (46 potential subjects aged 19-21 years). As shown in Table 3, 23 subjects were interviewed in FY 1979. The extensive 1979 Deutsch interview included items about school performance, motivation and values, family history, and general life adjustment as well as all items from the core 1979 Consortium interview. The Language Usage subtest of the Iowa Test of Basic Skills was also administered. In FY 1980, six case studies were forwarded to Cornell. The qualitative or case study approach was an attempt to provide a detailed picture of linkages between children's early experiences and their later life outcomes. Other investigators can use the case studies as important leads for further research and can draw on them for explanatory power. (A detailed discussion of these data is presented in Attachment F).

Gordon/Jester. Dr. Jester is carrying out the work of his deceased colleague, Dr. Ira Gordon. These subjects were 13 or 14 years old in the fall of 1979. Dr. Jester and his staff have collected the 1979 School Data information for 5 subjects and they have interviewed 22 subjects using a modified 1979 Youth Interview. STEP Locator tests in reading and mathematics have been administered to 24 subjects. Dr. Jester and his staff have determined the location of some subjects beyond the 107 subjects followed up in 1976 and hope to include them in this follow-up. For school competence analyses reported here, the source of the special education assignment and retention raw data was from Gordon/Jester computer files. This year-by-year data was sent to Ithaca in 1976. It covered kindergarten through 4th grade.

Gray. Subjects in the Early Training Project were 21 years old in the fall of 1979. Dr. Gray and her staff completed data collection and transmitted all data to the Ithaca office. They have completed the 1979 Youth Interviews and have transmitted that data as well as 1979 School Data Information. With an intensive effort, they located and interviewed or sent school data for 86 of their original 92 subjects; this was 9 more subjects than they found for the 1976 follow-up. Because records of psychological evaluations were unavailable they were unable to complete Part B for their sample. Details on school policies (Part C) were reported in correspondence over the past years.

Karnes. In 1980, an ex post facto control group of 58 subjects was constituted from children who were recruited for the Five Approaches Study but then not assigned to any preschool program. Development of a control group in 1980 has allowed more extensive utilization of Karnes' data than had been previously possible. For this reason, an attachment is included in this report which: (a) describes in detail the procedures followed in selecting program children and in developing the control group; (b) presents comparisons of the two groups on baseline measures; and (c) presents arguments for and against development of an ex post facto control group (see Attachment B). Comparisons of the treatment and control children on baseline measures (pretest IQ, mother's education, household SES, number of siblings, sex of subjects, father presence/absence and race) found only one significant difference; control children had more siblings than did program children at the time of pretesting. Generally, the two groups appear to be comparable and we can justify making program-control comparisons. These treatment and control subjects were 1 -19 years old in 1979. Dr. Karnes and her staff have collected School Record information (Part A) for 195 subjects. They updated the high school completion data in 1980 with information on school dropouts. They transmitted detailed information for 120 "problem" students who received special evaluations in their schools (Part B of School Data Form). In addition, the Karnes staff wrote a report describing the Champaign-Urbana school policies on dropout prevention programs and academic failure policies (Part C of School Data Form). Karnes' staff have actively located and interviewed those subjects (both treatment and controls) who were old enough to have completed high school. We have received 120 Youth Interviews thus far.

Levenstein. The decision was made to focus 1980 data collection exclusively on the 1968 program group who had had the full two years of the Levenstein program (T68 I & II, group 5) and their contemporaneous control group (group 19, the after-only controls). Dr. Levenstein's staff collected school data on 46 of these subjects (87%). These subjects were 14 years old in the fall of 1979.

Miller. These subjects were approximately 16 years old in fall 1979 and were thus legally eligible to drop out of high school. Dr. Miller and her staff have collected information on those subjects who dropped out in the 1979-80 academic year (N = 10). They contacted additonal subjects beyond those located for the 1976 follow-up and have collected up-to-date school record information for a total of 120 subjects. They administered the modified 1979 Youth Interview and have sent 156 interviews. The STEP locator achievement test scores for 91 students were received by the Ithaca office.

Palmer. Palmer's subjects are now 16 years old. Palmer's staff sent School Data Forms (Part A) for 219 subjects in FY1979. They collected 8th grade achievement tests scores from school records in FY1979. In FY1980, they collected 9th grade achievement test scores for 162 cases on three achievement tests.

Weikart. These subjects were 18-22 years old in FY1980. Dr. Weikart's staff continue to collect High/Scope data on their Perry Preschool subjects as each of the five waves turns 19 years old. They interviewed Wave 3 in 1980. School Data Forms (Part A) were sent for 53 cases (Wave 0, 1, and 2); these were subjects old enough to have completed high school. Information on certain school policies and practices with regard to special education placement and grade retention was forwarded. For school competence analyses, we used data on special education assignments and retention through 5th grade for 123 subjects. These data were published in Weber, Foster, and Weikart, 1978.

Data Management

Data management included consultation with field workers during data collection, receiving and checking raw data, office coding, keypunching, and data cleaning. School Record data received in FY 1979 and FY 1980 and the 1979-80 Interview data received to date have been merged into one large computer file along with the original data and data from the 1976 follow-up. This file contains over 890 variables and 900 cases. There are now three computer files: the file of original and IQ data for 3,522 cases, the 1976-1977 follow-up data on 2,079 cases, and the 1979-1980 follow-up with about 900 cases. The latter two files include most variables available from earlier files as well as all their respective follow-up data.

Interview data received in the fourth quarter are being prepared for addition to the files. The process of coding Karnes and Miller interviews has been completed with satisfactory coding reliability. Keypunching of these interviews is underway. Techniques have been devised and perfected to allow easy addition of new data to the computer files, since data will continue to arrive over the next few months for certain projects (e.g. Gordon/Jester project).

Problems and obstacles in data collection. One focus of our FY1980 data analyses has been the educational attainment of those subjects old enough to have graduated from high school (the subjects in the Beller, Gray, Karnes and Weikart projects who were scheduled to graduate in 1976-78). Data collection for the Beller and Gray subjects was essentially completed in FY1979. One set of Karnes' subjects graduated in June 1980, as expected; those data were forwarded to us and were included in our analyses. Some subjects in that cohort, however, did not graduate and were expected to register for school in Fall 1980. On the suggestion of the Karnes staff, it was decided that these subjects would not be categorized as dropouts; rather, categorization would wait until Fall 1980 school attendance records could be checked. This decision will increase the validity of our results, but has delayed the educational attainment analyses. Rather than eliminating Karnes' data from these analyses, we have included the data that were available; when additional cases are included, the results reported here will be revised. In addition, interviewing of the 18-19 year old subjects was postponed until they had a chance to complete school and begin or look for work.

Weikart's three oldest waves reached 12th grade by FY1980 but we were unable to include these in exploratory analyses because data was received after educational attainment analyses were completed.

Liaison with Subcontractors

One general Consortium meeting and one subcommittee meeting were held this year. A meeting of principal investigators or their representatives was held in Nashville on May 30-31, 1980. Present were E. Kuno Beller, Bernard Brown, Richard Darlington, Susan Gray, Robert Jester, Terry Jordan (representing the Deutsches), Merle Karnes, Irving Lazar, Phyllis Levenstein, Louise Miller Harry Murray, Barbara Ramsey, Jacqueline Royce, Vicki Shipman, Reuben Snipper, and David Weikart. Dr. Susan Gray and her staff made the on-site arrangements and the Ithaca staff coordinated travel arrangements and prepared the agenda. The group discussions were productive and raised important issues about the kinds of meta-analysis we in the Consortium are undertaking. At the meeting, a subcommittee was selected to bridge method and content issues. The subcommittee then met in Ithaca September 13-14. Irving Lazar, Merle Karnes, Susan Gray, Kuno Beller, Richard Darlington, and Vicki Shipman discussed the selection of research questions, variables for future analyses, and a proposed outline for the Consortium book.

In the process of monitoring data collection, extensive contact was maintained by telephone and mail with staff at each Consortium field site, averaging one contact per week per project. The Ithaca staff mailed a monthly report on the status of data collection, work completed, and work in progress to each field site.

WORK-IN-PROGRESS: ANALYSIS OF DATA (FY1980)

Implicit, and often explicit, in the rationales provided for early education programs is the expectation that investment in a positive early experience for children of low-income families can have immediate and later benefits. The Consortium study is a multisample meta-analysis that attempts to understand the impact and mechanisms of early education programs. Our goal is to construct analyses that relate the antecedents to the consequences of school success and failure, incorporating the influence of early education, family background and process measures, and measures of subjects' intellectual ability and social psychological attributes. The interrelationship of variables suggested by our data analyses thus far fit into a theoretical model of the process of achievement.

Theoretical Model

The Consortium data file contains variables collected over a 20-year period from early childhood to early adulthood. Measures were collected by Consortium members during five time periods: a) prior to entry in early education programs (family background and IQ test scores); b) at entry to school (IQ tests at age 6); c) throughout elementary and secondary schools year-by-year or selected years (data on school progression); d) at the end of high school (level of education completed); and e) one to four years post high school (e.g. labor market experiences).

The longitudinal Consortium data set with its time-ordered variables now provides an opportunity to test the role of early education in a theoretical model of achievement. The model we have begun to test with our data derives from models of the status attainment process and models of the schooling process found in sociology and economics. These models used in the study of social inequality and social stratification, describe the processes and events by which individuals come to occupy positions in the social hierarchy. According to the original Blau and Duncan study (1967), a sequence of major social statuses occurs over the course of an individual's socioeconomic life cycle. At every age or stage in the cycle, an individual's life chances, level of living, health and welfare, status and acceptance, and satisfaction and morale are influenced by the quality and level of the social status attained.

Status attainment models include primarily four sets of variables, (a) background characteristics which affect (b) educational attainment and eventually (c) occupational and (d) economic attainment. More recently, status attainment research has emphasized the intervening process between the family of origin and the future educational, occupational and economic attainments.

Two types of school process models have been under investigation. Social-psychological models focus on mechanisms of socialization and on their importance in shaping students' motivations and values (cf. Atkinson, Lens, & O'Malley, 1976; Sewell & Hauser, 1976; Walberg & Majoribanks, 1976). In contrast, other models emphasize the organizational and structural arrangements of schools (e.g. tracking) which influence educational outcomes independent of the interpersonal and subjective processes which are important in the social-psychological models (cf. Alexander, Cook & McDill, 1978; Karabel & Halsey, 1977; Rosenbaum, 1980).

The Consortium investigation of the role of early education on attainment has addressed two general questions: were there long-term effects? and were programs more effective for some subgroups of subjects than others? The theoretical model we have conceptualized with our data tests the role of early education on four outcome variables over time. The main outcome variables being considered are early mental ability (age 6), school competence (measured in several ways), educational attainment (also measured in several ways), and labor market experience. These outcome variables are important events in the achievement or status attainment process and have critical implications for an individual's ultimate occupational and economic attainment. Each outcome variable in our model becomes an independent variable or a tecedent at the next time period. The sources of data for the dependent variables are primarily the 1979 School Data forms and interviews; for the independent variables, the data are primarily the 1976 and 1979 interviews and the 1979 School Data forms.

The underlying research questions can be stated symbolically as a series of equations to be tested statistically with multiple regression techniques. The equations for the four dependent variables are listed below. The independent variables are shown on the right side of the equation. Detailed descriptions of the theoretical model, system of equations, and variables in the model can be found in Attachment C.

Equation 1. Mental Ability at Age 6 = Early Education + SEX + SES + Household Composition + Interactions of Early Education with Background Variables

Equation 2. School Competence = Early Education + SEX + SES + Household Composition + Early Mental Ability + Interactions of early Education with Background Variables

Equation 3. Educational Attainment = Early Education + SEX + SES + Household Composition + Early Mental Ability + School Competence + Suspensions + Pregnancy during High School + High School Track + Attitudes and Values of Subject + Family Social Psychological Environment + Interactions of Early Education and Selected Independent Variables

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Equation 4. Labor Market Outcomes = Early Education + SEX + SES + Household Composition + Early Mental Ability + School Competence + Educational Attainment + Suspensions + Subject Achievement-Orientation + Previous Work Experience + Interactions of Early Education and Selected Independent Variables

In FY1980, we made explicit our working theoretical model with its intervening variables; we fit the analyses we had completed by FY1979 into this framework (equation 1 - early mental ability and equation 2 - school competence); and began using the 1979-80 data to refine schooling analyses and extend our study of the effects of early education to later outcomes (equation 3 - educational attainment and equation 4 - labor market outco

Research Questions and Overview of Data Analyses

By September 30, 1980 we had sufficient data collected, cleaned, coded and on computer file to permit analyses pertinent to the following research questions:

1. Educational attainment. Are participants in early education programs more likely to complete high school than are control subjects?

Are program subjects more likely to attend college after graduation? Are program subjects more likely than control subjects to receive post-high school training, whether or not they completed high school? The preparations for data analysis required the examination of five different data sources. The lengthy process of reconciliation among these sources allowed us to have confidence in the reliability of these data. Three educational attainment outcome variables were created: (a) a dichotomous variable of high school completion vs. non-completion; (b) a 3-point scale of high school non-completion, completion, and college attendance; and (c) a 5-point scale which included data on non-academic training, such as vocational school, on-the-job and armed services training. Preliminary analyses were completed in FY1980 and are presented in this report. More extensive analyses will be undertaken in FY1981.

2. School competence (special education placement and grade retention). Do control subjects show problems earlier in their school careers than do program subjects? Once a student starts to have school difficulties, does a control subject have more difficulty than a program subject staying on-grade and achieving standard progression? For example, do they spend more years in special classes than do program subjects? Year-by-year data is now on computer file; we have completed work on specifying the dependent variable, and have written the computer program to use this dependent variable over time. In this report we present descriptive graphs; statistical analyses were begun and will continue next year.

3. Other schooling variables (course marks, track placement, suspensions). Do control subjects have lower course marks and at what grade levels? Are there differences between program and control subjects in terms of placement in regular, college preparatory, or vocational tracks in the high school years? Do control subjects have

more suspensions (as reported on school records and self-reports) than do program subjects? In preparation for data analyses, we operationally defined these three dependent variables. This process involved difficult conceptual and methodological issues, such as the meaning of track placements for retained subjects, the reconciliation of discrepancies between the self-reports and school records on suspension, and the diversity of schemes for recording course marks. Preliminary statistical analyses relating early education to course marks and track placements are presented in this report. More in-depth analyses of these relationships will take place next year. Analyses using suspension data are currently underway.

[illegible]. Labor market experiences. Do program subjects differ from controls in rates of labor force participation? Are they more likely to show economic independence than controls; that is, do they differ on source of income? Are there program/control differences in length of employment, job satisfaction, monthly earnings? In the past year we have begun the definition of labor market variables from data in the 1979-1980 youth interview. There are important social considerations in defining labor market outcomes for low-income minority youth. As in the past, we have searched for ecologically valid indicators which have a meaning in the context of the subjects' own lives. Our preliminary definitions of labor market dependent variables are presented in Attachment C. Data analyses will begin in FY1981.

5. Predicting program effectiveness. Considerable time during this past year was devoted to developing a strategy for the analysis of program characteristics. The question of which program characteristics best predict program effectiveness is an important one for program designers. It addresses the central issue of how early education exerts its effects. In previous approaches to this question we encountered difficulties in

using individuals as the unit of analysis as well as in constructing comparable measures of effectiveness. The current effort aimed to use the curriculum approach as the unit of analysis, to construct new descriptions of program variation, to code the three projects that were specifically designed to compare different approaches, and to use dependent variables that were strictly comparable and could thus be pooled. The three projects were Karnes' Five Approaches, Miller's Head Start Variations, and Weikart's Curriculum Demonstration project. There were a total of 12 different program approaches across the three projects. After consultation with the staff at the three projects we agreed there were six dimensions with codeable variability across programs: a) role of the teacher in the classroom; b) nature of group experience; c) equipment and materials; d) program objectives; e) sequencing of instructional materials; and f) emphasis on language. Subsequent in-depth consultations with Louise Miller revealed methodological problems that would limit our ability to draw valid conclusions from program characteristics analyses. In spite of the considerable time devoted by the Ithaca staff to this very important question, it is clear that redoubled efforts will not solve these problems. The Ithaca staff is planning to write, during FY1981, a discussion of the problems encountered.

In the next sections we present the analyses related to these questions using the data which was on computer file as of September 30, 1980. Some projects are still collecting data so these results must be considered preliminary.

Effect of Early Education on Educational Attainment: Overview of Education Attainment Literature

The educational attainment analyses, although preliminary, are particularly important in that they are the first to address Equation 3 of our model. It is appropriate here, then, to include a brief examination of previous research on the antecedents and consequences of educational attainment. This will place our investigation in the context of current thinking in the fields of economics, psychology, education, and social policy.

One major concern during this stage of the analyses has been the identification of antecedent characteristics associated with educational attainment, particularly high school completion. Status attainment and human capital theory models have attempted to explain achievement in schooling, as well as success in the labor market, on the basis of characteristics such as mental ability, socioeconomic background, influence of significant others', educational and occupational aspirations, quality of high school attended, curriculum choices, and knowledge of the world of work (e.g. Blau and Duncan, 1967; Sewell and Hauser, 1975; Hauser and Featherman, 1977; Becker, 1975; Mencer, 1974; Wallace and Ihnon, 1975).

Hill (1978), using human capital theory to develop a structure system model for predicting the decision to drop out of high school, developed a set of equations to account for an individual's ability, his knowledge of the prevailing labor market conditions, his curriculum track in high school and the effect of these variables on the decision to drop out. Sewell and Hauser (1972, 1975) extended the original status attainment model developed by Blau and Duncan (1967). By incorporating several social psychological variables such as academic performance, perceived expectations of significant others, and educational and occupational aspirations, they succeeded in accounting for a large portion of the total variance of educational attainment.

Several studies based on these models have documented black/white differences in the dynamics of educational attainment. Hill (1978), using National Longitudinal Survey data of 14-17 year olds in 1966, found that some factors in the educational attainment process followed different channels for whites and nonwhites. For instance, for whites, mental ability had the most important effect on dropping out, while for nonwhites, knowledge of the world of work was the most important explanatory variable. With regard to socioeconomic background variables, for nonwhites, only mother's education had a significant direct effect on dropping out, while for whites, all four of the selected variables had significant effects on dropping out (father's education, mother's education, number of siblings and the three-year average income of the family). Blau & Duncan (1967) and Duncan (1968) documented the existence of a "double handicap" for blacks in that parental status attainment failed to be transmitted to the new generation, and individual's educational attainment failed to convert to occupational achievement and income. Jencks, et al. (1972) as part of a larger study, looked into the interaction effect of race with other variables and found that after socioeconomic background and ability were controlled for, being black had a positive effect on educational aspirations and educational attainment. In an extension of these studies, Portes and Wilson (1976) found that the positive effect of being black, after socioeconomic background and ability were controlled for, appeared to be entirely mediated by the following factors: academic performance, self-esteem, and influences from significant others.

In contrast to the theoretically-based research in economics and sociology, there is a large body of education literature which also attempts to identify antecendents of high school completion or dropping out. Most of these studies had two major weaknesses: large numbers of variables were included without theoretical justification; and the methods used were correlational in nature so that causal sequences were not addressed. Neither were interaction effects investigated. Nevertheless, many types of variables have been shown to be associated with failure to complete high school; these can be categorized as family background and process variables, academic performance and disciplinary problems.

Parents of dropouts, in comparison to parents of graduates, tended to have lower educational levels (Tseng, 1972; Lloyd & Beach, 1972), lower occupational levels, and lower incomes (Los Angeles Unified School District, 1974). Dropouts have been found to have significantly more siblings than graduates (Delaney & Tovian, 1972) and, for whites, more divorced or separated parents (Lloyd & Beach, 1972).

Many studies have investigated parental attitudes as a factor in dropping out of high school. It has been found that dropping out was associated with less parental encouragement of educational achievement (Tseng, 1972) and lower parental expectations for achievement, among males (Tucker, 1979). Investigations of family relationships have found that: the better the child got along with his/her parents, the more likely he/she was to finish high school and enter college (Bachman et al, 1972); dropouts perceived lower acceptance and higher avoidance by their fathers (Tseng, 1972); dropouts reported a "lack of emotional support" at home (Hunt & Woods, 1979); and dropouts tended not to use their parents as referents (Tucker, 1979). Unfortunately, many of these studies were based on cross-sectional data, in which case the arguments are potentially circular; parental attitudes could easily be a result of their children's poor performance in high school prior to dropping out.

As might be expected, those who did not complete high school were found to have experienced more difficulty in school than eventual graduates. Many studies have found that two important predictors of dropping-out were low school grades and grade retention (Delaney & Tovian, 1972; Washington, 1973; Spencer, 1977; Hunt & Woods, 1979). Schuster (1971) found that the average dropout was two years behind the average eventual graduate in reading scores, and Lloyd & Beach (1972) also found significantly lower reading abilities for dropouts as well as lower arithmetic and language skill levels (this latter finding applied to whites only). Lloyd & Beach (1972), among others, found that IQ score was a significant predictor of dropping out, and Tseng, (1972) found significant deficits in dropouts' scores on a measure of subjects' accuracy of perception of the occupational prestige hierarchy. Studies which looked into students' attitides and motivations found dropouts showed less interest in school (Los Angeles Unified School District, 1974), lower levels of achievement motivation, lower occupational aspirations, and a tendency to choose future jobs with lower levels of difficulty, responsibility and prestige than eventual graduates (Tseng, 1972). Dropouts have been characterized, while in school, as having more disciplinary problems (Elliot & Voss as reported in Neill, 1979) and higher rates of absenteeism than eventual graduates. They were less likely to participate in extracurricular activities (Knudsen 1964). When studied during high school, eventual dropouts have been characterized in social psychological terms as submissive, introverted, emotional, and as being low in aspiration, anxiety, tension and creativity (Delaney & Goodyear, 1972), and subject to "negative influences from peers" (Hunt & Woods, 1979).

A few studies have looked at antecedents of completion/noncompletion of high school which, having been measured much earlier than those presented above, are somewhat more immune to the problem of circularity of causality. Stroup & Robins (1972) argued that the signs of impending dropout were visible in elementary school, and included early failure, excessive absence, delinquency, drinking, interschool mobility and low family encouragement. Stringer (1973) contended that "high absences" and "poor achievement" in elementary school were significant predictors of dropping out, while Lloyd & Beach (1972) using third grade data, found that later dropouts were significantly lower than later graduates on reading scores, IQ levels, and marks received in coursework. (In their white subsample, dropouts also differed from graduates in parents' educational and occupational level, family size, marital status of parents and tested arithmetic and language skill levels).

The present study will extend our knowledge of the antecedents of dropping out (as summarized in this review of the literature) in several important ways. First, its focus is on low-income black youth, both male and female, whereas many of the studies cited either focused on white males or have had samples of blacks too small for generalization of results. Secondly, our extensive family background and school record data, collected at four points in time (starting when the subjects were four years old) will allow assessment of the effects of antecedent variables, their possible interaction effects, and their causal sequences.

Educational attainment has been extensively studied because it is an important intermediate variable in the status attainment process. The relationship between high school completion and later labor market experience is complex. It is affected by many factors, especially the prevailing economic conditions in the larger society and the stage of the individual's life cycle being investigated. The Youth in Transition (Bachman, et al., 1971) and Project Talent (Combs & Cooley, 1968) studies found few or no negative consequences for male dropouts compared to graduates with respect to unemployment, particularly after controlling for ability and family background. However, occupational success was measured when the youths were only 19-20 years old, and both studies were performed at a time when labor supply matched demand (i.e., tight labor market) which is not true today. Studies which followed subjects for a longer period of time have found the expected earning differential between dropouts and graduates. Blau & Duncan (1967) and Sewell and Hauser (1972) both found that high school graduation had a positive effect on subjects' early as well as later occupational status. King (1978), using National Longitudinal Survey data, found that earning differences were not pronounced immediately upon leaving school, but became significant over a thirteen year period of study. It was also found that graduates enjoyed a greater immunity to unemployment than dropouts, and this increased over time. King also found that high school graduates were more likely to have received post-high school training and to have built up greater job tenure. Finally, he found that the position of dropouts relative to graduates in many of these measures was substantially worse in a labor market where supply exceeds demand, such as prevails today.

To summarize these findings, there is a clear positive effect of high school completion on labor market outcomes. The extent of this effect is highly dependent on the economic climate. The difference between high school graduates and dropouts, in terms of earnings and ranking of occupations, tends to increase as the life cycle goes on. It is reasonable to assume that with the current high unemployment rate for low-income black youth, high school completion will have a stronger effect on labor market outcomes than has been found in earlier research. Future Consortium work will attempt to clarify the specific effects of educational attainment on labor market outcomes, and the role of early education in this effect.

Methods

Our major question in these analyses was whether preschool programs had any effects on students' later educational attainment. Also examined were the relationships between educational attainment and other antecedent variables including sex, mother's educational attainment and whether the student maintained standard school progression. For the three dependent variables in this study, only effects on high school completion, the dichotomous variable, will be examined by inferential statistical procedures. The 3-point and 5-point educational attainment scales are still in the process of refinement so that only the descriptive statistics will be reported here. In the future we will address the question whether early education was a significant factor in the completion of high school for certain types of subjects.

These findings reported here should be considered preliminary for several reasons. Analyses were performed on only three of the Consortium projects, those whose subjects were old enough to be eligible for high school graduation. Information for some additional cases in one of the three projects will be included in future analyses. The effect of preschool program on high school completion was examined twice -- once using a chi-square test, and again using multiple regression to control for background and other variables (gender, mother's education level, father presence vs. absence, number of siblings, and preprogram IQ score). Multiple regression has been used in previous analyses to ensure that the effect of program on outcome measures was not due to differences between program and control groups on background characteristics.

Our second research question was whether gender, mother's educational attainment, and students' maintenance of standard school progression had any effect on high school completion. Simple contingency analyses were performed for all three variables. Gender difference was also examined by multiple regression with program effect, preprogram IQ score, and other background variables controlled. These three variables could play the role of intervening variables between early education programs and outcome measures. This possibility will be explored in future analyses.

After results for each of the three projects had been examined, they were then combined. For descriptive statistics (e.g. percentages), the unweighted mean score was reported so as to give equal weight to each project. The pooled-z technique was the inferential statistical procedure used. The procedures for this technique have been described in detail in other publications (see draft of monograph, Attachment E).

Definition of variables. Three dependent variables were developed as measures of subjects' educational attainment. The first was a simple dichotomous variable: completion/noncompletion of high school. Completion of high school was defined as having graduated from high school or having passed a high school equivalency test. A second dependent variable was a 3-point educational attainment scale: the three categories were high school dropout, high school graduate with no college, and high school graduate with some college. A third dependent variable consisted of five categories of educational attainment: high school dropout with no further training; high school dropout with some out-of-school training; high school graduate with no further training; high school graduate with some out-of-school training but no college education; and high school graduate with some college education. Out-of-school training was operationalized as on-the-job training, vocational school training, training in armed services, or attending but not completing a high school equivalency program.

The majority of the independent variables included in these analyses were the same background variables used in previous analyses. They were program-control status, gender, mother's educational level, father presence, number of siblings, and preprogram IQ score (see draft of SRCD Monograph in attachments for full descriptions of each variable). Two other independent variables were developed specifically for the educational attainment analyses. The first was a dichotomous variable, mother's completion/noncompletion of high school. For some comparisons, this variable was further differentiated to form three categories: high school dropout; high school graduate; and high school graduate with some college. The second was also a dichotomous variable, whether or not the student maintained standard school progression. A student was defined as having maintained standard school progression if he/she was never retained in a grade and was never placed in a special education class. In standard school progression analyses, one subset of the subjects was not included; these were the subjects who were assigned to special education classes early in their school careers (generally, by third or fourth grade) *and* who remained in these classes until graduation or dropping out.

Data sources. Data for the three educational attainment scales (dichotomous, 3-point and 5-point) were drawn from the 1979 School Data Forms, Part A, and the 1979 Youth Interviews. For approximately 20% of Karnes' subjects (both program and control groups) these data were not available; for those subjects the assumption was made that they received no additional training and did not attend college. Obviously, then, analyses for Karnes' Project using the 3-point or 5-point educational attainment scales must be considered preliminary. Any discrepancies between the two data sources were reconciled through discussions with the individual investigators. Data for the background variables (mother's education, father presence, number of siblings) were collected, for Gray and Karnes, at the time of acceptance into the early education project. Beller collected these data retrospectively, as part of the 1976 Parent Interview. Mother's high school completion data were based on the 1976 Parent Interview for the Beller and Gray projects. Karnes' control group was developed in 1980 and no 1976 interviews were conducted; mother's high school completion status for Karnes was developed from the original (preprogram) data for both program and control groups in order to compare the two groups on an equal basis. The data for standard school progression were drawn from the 1980 School Data Form (Part A).

Groups included in analyses. Data from three projects, those of Beller, Gray and Karnes, were included in the present analyses. Projects, and groups within projects, were selected on the basis of three criteria: (a) subjects were old enough to have graduated from high school; (b) data collection had been substantially completed by August, 1980; and (c) those program and control groups, within a project, that most closely approximated the same population and thus provided the most rigorous comparison. For Beller's project, the data utilized were those of the nursery and kindergarten groups; for Gray's project, the two experimental groups and the local control group were used; for Karnes' project the groups in the Five Approaches study and their control counterparts (Waves 1 and 2) were included. This is the first year that Karnes' project data have been used extensively in analyses; development of an ex post facto control group has made this possible (see attachment B of this report for discussions of the Five Approaches program groups, sample selection procedures, arguments for and against the development of post hoc control groups, and the procedures followed in developing this group.

Results

In this section we will first report the findings on our major research question: Does participation in early education programs have an effect on later educational attainment particularly high school completion ? Then we address a secondary problem of investigating other variables which affect educational attainment.

The effect of early education program on high school completion is shown in Tables 4 and 5. All three projects reported a higher percent of high school graduates among the program subjects than among the control subjects. Summarizing over the three projects, 69.2% of the program children completed high school, while 57.7% of the control children completed high school (Table 4). Using 2 x 2 chi-square tests, program/control differences were not significant for the three projects individually (the two-tailed $\underline{p}$ values ranged from .270 to .419). The combined result for the three projects did not reach significance (pooled $\underline{z} = 1.72$, pooled $\underline{p} = .086$, two-tailed). These results became stronger when we used multiple regression as shown in Table 5. Although program and control groups generally did not differ significantly on background or demographic variables measured before program entry, standard multiple regression was used to control for whatever differences were observed. Covariates controlled in this way were: level of maternal education (in grades completed), father presence versus absence, number of siblings, gender, and preprogram IQ score. Regression analyses on the dichotomous scale high school completion/dropout, was close to significant (pooled $\underline{z} = 1.95$, p = two-tailed .051). Thus we can conclude, when background differences were controlled, children benefitted from early education programs such that they were more likely to have completed high school (Table 5).

In Table 6, program/control data are presented for the 3-category educational attainment variable. The additional information is whether or not the students attended some college. Column 1 presents the complementary percents of Table 4, revealing a tendency for control subjects to be more likely to have dropped out of high school than program subjects. What is indicated in column 3 is that control children were as likely to attend college as were program children, however, the necessary statistical tests have not yet been performed.

When the educational attainment data are divided into 5 categories, thus providing information on out-of-school training, the raw percentages are shown in Table 7. Future analyses will investigate effects of early education on out-of-school training for both dropouts and high school graduates. In Tables 8 through 13, we report findings on the effect of gender, mother's educational attainment, and school performance on high school completion. Gender differences in high school completion are shown in Table 8. In these samples, with program and control subjects combined, females were more likely to graduate from high school than males. Across the three projects with program and control subjects combined, 74.6% of the females completed high school, while 56.1% of the males did so. Results of chi-square tests were close to significant in two of the projects (p values were .058 , .086 and .122). When results were pooled, the difference was highly significant (z = 2.98, p = .003). Multiple regression was used to examine the female-male difference on high school completion when program effect, preprogram IQ and other background variables were controlled (Table 9). The results confirm those of the chi-square tests -- females were significantly more likely to graduate from high school than were males, when background variables were controlled (pooled z = 2.64, p = .008).

Previous research has consistently found parents' educational attainment status to be an important factor in predicting children's educational attainment. Preliminary analyses have found this relationship in the present data. When the data within each project were sorted dichotomously by mother's education (completed high school vs. not), and chi-square tests were performed, trends were apparent in two of the projects (Table 10). Over the three projects, 78.7% of the students whose mothers did complete high school, also completed high school. Of the students whose mothers did not complete high school, only 58.4% completed high school. When results of the three projects were pooled, the difference was significant (pooled $z = 2.76$, $p = .006$). These results reveal no surprises -- students whose mothers have completed high school are themselves more likely to have completed high school. In Table 11 these percentages are presented in a more detailed fashion. Three categories of mothers' education (not completed high school, completed high school, some college) were crosstabulated with five categories of student educational attainment (dropout with no out-of-school training, dropout with some training, high school graduate with no post-high school training, graduate with some training, some college). No statisitical tests have been performed using these categories; the number of subjects in some cells was too small to provide meaningful percentages (see particularly, mothers with some college). Future analyses will explore more fully the relationships among a student's early education experience, and later educational attainment and mother's educational attainment.

Another strong predictor of high school completion, as found in the educational literature, has been school performance. We have performed preliminary analyses on the relationship between school performance and educational attainment by using a dichotomous measure of school performance -- whether or not the student had maintained standard school progression (which we have defined as never having been retained in grade or assigned to special education classes vs having been retained or assigned). In Tables 12 and 13, this variable is labelled Achiever and Nonachiever. Across the three projects, 89.4% of the Achievers graduated from high school, while only 38.3% of the Nonachievers graduated (Table 12). Within each project, the correlation between achiever status and high school completion was highly significant ($p < .001$ for each project). The correlations were .426, .567, and .573 for Karnes, Gray's, and Bellers projects, respectively. Students who were never retained in grade or assigned to special education were more likely to complete high school than those students who were unable to maintain this standard school progression. The 5-category educational attainment distribution by achiever status is presented in Table 13. Although no summary or hypothesis-testing statistics were calculated, there is a perceptible relationship between achiever status and college attendance within each of the three projects. (Again, it is important to recall that Karnes' data are incomplete on college attendance.) In Beller's and Gray's projects, fully 40% of the Achievers attended college for some period of time. A surprising 17% of Gray's Nonachievers also attended some college. Further analyses are being undertaken to lay out the relationships among early education, achiever status, and high school completion.

To summarize the preliminary analyses regarding educational attainment, several statements can be made. Early education of children from low-income families appeared to have a real effect on the likelihood of completing high school. The effect of early education appears not to have extended to college attendance. With regard to identification of confounding or intervening variables, the data provide three clear indicators. First, there is a distinct male-female difference in high school completion rate. Secondly, students whose mothers graduated from high school were themselves more likely to graduate from high school; this relationship may possibly extend to college attendence. Finally, students who had maintained standard school progression were more likely to graduate from high school than were students who were retained in grade or assigned to one or more special education classes.

Table 4

Percent of Students Who Completed High School

Program vs. Control

Project (N)	Program	Control	Chi-Square	p (2-tailed)	Pooled z	Pooled p (2-tailed)
Beller (79)	69.8%	61.1%	.65	.419		
Gray (59)	67.5	52.6	1.22	.270		
Karnes (111)	70.2	59.3	1.12	.289		
Mean (unweighted)	69.2	57.7			1.7156	.0862

Table 5

Effect of Early Education on High School Completion
(Background Variables and Pretest IQ Score Controlled)

Project	n	Program Coefficient	F	Pooled z	Pooled p (2-tail)
Beller	61	.095	.586		
Gray	58	.149	1.338		
Karnes	95	.203	2.263		
				1.9522	.0509

Table 6

Students' Educational Attainment Status
(3-point scale)
Program vs. Control

Project	Program vs Control	(1) Dropout	(2) Graduate with no College	(3) Some College
Beller (79)	Program (43)	30.2	46.5	23.3
	Control (36)	38.9	33.4	27.8
Gray (59)	Program (40)	32.5	40.0	27.5
	Control (19)	47.4	26.3	26.3
Karnes (111)	Program (84)	29.8	58.4	11.9
	Control (27)	40.7	37.0	22.2

Table 7

Students' Educational Attainment Status
(5-Point Scale)
Program vs. Control

Project	(1) Dropout with no Training	(2) Dropout with some Training	(3) Graduate with no Training	(4) Graduate with Training	(5) Some College
Beller (79)					
Program (43)	20.9	9.3	30.2	16.3	23.3
Control (36)	25.0	13.9	27.8	5.6	27.8
Gray (59)					
Program (40)	25.0	7.5	22.5	17.5	27.5
Control (19)	42.1	5.3	15.8	10.5	26.3
Karnes (111)					
Program (84)	23.8	6.0	53.6	4.8	11.9
Control (27)	33.3	7.4	33.3	3.7	22.2

Table 8

Percent of Students Who Completed High School

Male vs Female

Project (N)	Male	Female	Chi-Square	p (2-tailed)	Pooled z	Pooled p (2-tailed)
Beller (79)	56.1	76.3	3.58	.058		
Gray (59)	51.7	73.3	2.94	.086		
Karnes (111)	60.4	74.1	2.39	.122		
Mean (unweighted)	56.1	74.6			2.9793	.0029

Table 9

Effect of Gender on High School Completion
(Background Variables, Present IQ Score, and Program Effect Controlled)

Project	n	Sex Coefficient	F	Pooled *z*	Pooled *p*
Beller	61	.264	4.763	2.639	.0083
Gray	58	.123	.952		
Karnes	95	.151	2.277		

Table 10

Percent of Students Who Completed High School
Mother Completed High School vs. Not

Project (n)	Mother completed high school	Mother did not complete high school	Chi-square	p(2-tailed)	Pooled _z_	Pooled _p_
Beller (65)	76.5	54.8	3.39	.066		
Gray (49)	84.6	55.6	3.47	.063		
Karnes (104)	75.0	64.7	1.15	.283		
Mean (unweighted)	78.7	58.4			2.7552	.0059

*Mother's high school completion status based on 1976 follow-up data for Beller and Gray. For Karnes, there were no 1976 follow-up data for control subjects; mother's high school completion based on pretest data.

Table 11

Students' Educational Attainment Status (5-Point Scale) Mothers Not Completed High School vs. Completed High School vs. Some College

Project	Mothers' Educational Status	(1) Dropout with no Training	(2) Dropout with Training	(3) Graduate with no Training	(4) Graduate with Training	(5) Some College
Beller (65)	Mother did not complete H.S. (31)	29.0	16.1	19.4	16.1	19.4
	Mother completed H.S. only (25)	16.0	8.0	32.0	8.0	36.0
	Mother had some College (9)	11.1	11.1	33.3	0.0	44.4
Gray (49)	Mother did not complete H.S. (36)	38.9	5.6	16.7	16.7	22.2
	Mother completed H.S. only (9)	22.2	0.0	22.2	22.2	33.3
	Mother had some College (4)	0.0	0.0	50.0	25.0	25.0
Karnes (104)	Mother did not complete H.S. (68)	26.5	8.8	47.1	7.4	10.3
	Mother completed H.S. only (30)	20.0	0.0	56.7	0.0	23.3
	Mother had some	33.3	16.7	16.7	0.0	33.3

Table 12

Percent of Students Who Completed High School
Achiever vs. Not*

Project (n)	Achiever	Non-achiever	r	p(2-tailed)
Beller (79)	90.7	36.1	.573	$<.001$
Gray (59)	95.8	40.0	.567	$<.001$
Karnes (102)	81.7	38.7	.426	$<.001$
Mean (unweighted)	89.4	38.3		

*Achiever is defined as never retained in grade or placed in special education classes. Subjects who were assigned to special educational classes early in their school careers and stayed in these classes until graduation or dropping out were not included in the analysis.

Table 13

Students' Educational Attainment Status (5-point scale)
Achiever vs. Not*

Project	Achiever vs. Not	(1) Dropout with no Training	(2) Dropout with Training	(3) Graduate with no Training	(4) Graduate with Training	(5) Some College
Beller (79)	Ach (43)	9.3	0.0	37.2	11.6	41.9
	NonAch (36)	38.9	25.0	19.4	11.1	5.6
Gray (59)	Ach (24)	4.2	0.0	25.0	29.2	41.7
	NonAch (35)	48.6	11.4	17.1	5.7	17.1
Karnes (102)	Ach (71)	16.9	1.4	53.5	7.0	21.1
	NonAch (31)	45.2	16.1	38.7	0.0	0.0

*Achiever is defined as never retained in grade or placed in special education classes. Subjects who were assigned to special education classes early in their school careers and stayed in these classes until graduation or dropping out were not included in the analysis.

The Effect of Early Education on School Competence: School Progression

Success or failure in meeting school requirements has important consequences for an individual's ultimate educational and occupational attainment. Recognizing this, a basic goal of all early education programs for low-income children has been to improve competence in school. In order to evaluate whether early education improves school competence, the Consortium members previously collected and analyzed indirect measures widely used to predict school performance such as IQ and other standardized tests (Consortium, 1977, 1978, in press). But various direct, ecologically valid measures were also collected in 1976-77 and in 1979-80.

In FY1980 we extended our analyses of special education placements and grade retention and then added two new variables collected in the 1979-80 follow-up. -- tracking and course marks. These three outcome measures provided an assessment of an individual's ability to function in their own social setting. They are, we feel, global indicators of an individual's adaption to expectations in the school milieu. In the next sections we report on our preliminary analyses on three types of school competence variables: school progression (special education placements and retentions), tracking, and course marks.

School progression: previous fundings on special education and retention

In the 1976-1977 follow-up, school records were examined to determine whether the subjects were ever assigned to special education classes or retained in grade at any time during their school careers. Data for some projects was complete through 12th grade; other projects completed data collection through earlier grades, generally through 5th to 7th grade.

Previously reported analyses of the data gathered in the 1976-1977 follow-up show long-term positive effects for preschool participants on rate of assignment to special education classes and on rate of grade retention (Consortium 1977, 1978, in press). These effects were statistically significant and substantial: the median rate of assignment to special education classes for program children was half the rate of controls (14% vs. 29%). We subjected these findings to various tests of alternative hypotheses including controlling for background variables, preprogram IQ scores and IQ scores at age 6. Extensive attrition analyses were run to check for possible attrition bias. In addition, logit analyses were performed to compare the results obtained by a procedure designed specifically for dichotomous dependent variables with those obtained by the multiple regression procedures used previously. The results remained significant and unchanged.

In an effort to extend these analyses, for the 1979-1980 follow-up, the Consortium members collected detailed yearly data from school records. The yearly data provide information on the entire school career and allow in-depth analyses of program/control differences in differential rates of assignment to special education and grade retention for each grade as well as providing estimates of the pattern and timing of these events. Furthermore, these data could, we felt, provide insights into the whole process of how early education affects school success or failure and later outcomes such as high school completion.

Concept Operationalization

Data by grade level allows a finer grained analysis than was possible before, but analyzing time series data is methodologically difficult. The first task is posing an appropriate research question. We now have data to address questions such as whether control subjects show problems earlier in their school career than do program graduates, or whether once an individual starts to have school difficulties, do controls have more difficulty staying on-grade, or whether controls spend more years in special education classes than project subjects.

There are a large number of possible research hypotheses and the operationalization of the concepts follows from the research questions. For purposes of initial description, we developed four slightly different operationalizations for the general concept of nonstandard school progression or assignment to some "school problem" status. The particular hypothesis one wants to test determines which operationalization is most appropriate. The four possible operationalizations will be discussed below. We selected one for analyses this year, and those results are presented in this report.

With data for each year it is possible to estimate not only the size but also the persistence of the effect of early education. In order to do this for a given dependent variable, an analysis is done for each grade level. As before for the school competence analysis, the focus is on three dependent variables: a) assignment to special education classes, b) grade retention and c) a composite variable indicating some deviation from standard school progress (for example, special education placement, grade retention, incomplete years, dropping out, etc.). Thus for example, special education assignment can be measured in four ways and for every grade. Four possible operationalizations of each dependent variable are described below.

1. First assignment to " school problem" status by given grade. This analyses is concerned with whether controls have school problems earlier in their school career than program subjects. The dependent variable is simply a dichotomous variable (yes, no) asking whether this grade is the first assignment to "school problem" status. This measure is computed for each grade. For first assignment at given grade to be meaningful in the analysis for a given grade, those cases which have been

assigned at an earlier grade are excluded. Thus the number of cases declines as the analysis proceeds through the grades because the children who are "at risk" of being assigned declines as some children are actually assigned. High school dropouts are excluded from the analysis for grades after they drop out. If a dropout returns, the case in included in the analysis. This measure is useful for analyses, such as the method of event histories, which focus on the timing of the first occurrence of assignment.

2. Ever assigned to "school problem status" by given grade. This conceptualization is most clearly related to our previous analyses where the dependent variable was ever assigned (to special education classes) up to the grade level when data were collected. The dependent variable in the grade-by-grade analyses we can now do is a dichotomous variable (yes, no) asking whether this case has ever been assigned by this grade. This measure is computed for each grade. In this instance, those cases which have been assigned at an earlier grade are still in the analysis at all later grade levels With regard to high school dropouts and whether they are included in the analyses at grades after they dropped out, there are two possibilities: if they were previously assigned before dropping out, then they are included in the analysis; if they were never assigned, the choice of whether to include them is problematic but we chose to include them in the analysis also. This measure focuses on the cumulative experience of the children and complements the first-assignment-by-given-grade measure, described in the preceding paragraph.

Because this operationalization is conceptually similar to our previous analyses (Consortium, 1978, in press) we started these analyses after sufficient data was on computer file. The preliminary results are presented in this report.

3. Currently assigned to "school problem status" at given grade. Developmental questions are concerned with the frequency of nonstandard

progression in a particular grade. Program/control differences can be explored. The dependent variable is a dichotomous variable (yes, no) asking whether this case is currently assigned to "school problem" status at this grade. This measure is computed for each grade. High school dropouts are excluded for the given grade.

4. Time spent assigned to "school problem" status up through given grade. For this analysis, the dependent variable is a percent indicating the fraction of possible years that this case has been assigned to "school problem" status up through this grade. This measure is computed for each grade. For this procedure adjustments have to be made for the true number of possible years of assignment. Thus for grade retention, an additional year is added to the possible years; for years dropped, an additional year is subtracted. Dropouts present special problems as before so we will investigate the sensitivity of the analyses to various ways of handling dropouts. This measure has direct policy relevance because of the possibility of estimating the reduction in resources used which results from early education programs.

Originally, we also planned to investigate differential patterns of assignment between the program and control children, but the cell sizes proved to small for statistical analysis. We will describe the general features of the different patterns of school careers. Questions of interest in this context include whether treatment children, once assigned, return to normal progress more often than controls, and whether they return sooner than controls.

Methods

Definitions of Variables. In this section, we specify the exact operationalizations of the variables used in the analyses presented in this report. Special education classes were defined as unspecified special education, trainable or educable mentally retarded, emotionally disturbed or behavior disorder, and learning disability. Not in special education this report. Special education classes were defined as unspecified special education, trainable or educable mentally retarded, emotionally disturbed or behavior disorder, and learning disability. Not in special education classes included regular classes, gifted classes, speech and/or hearing classes, and classes for the orthopedically and perceptually handicapped. Retention in grade was defined as not promoted (i.e. repeated the entire year). Not retained included social promotion, retained part of the year (but still promoted at the end of the year to the next grade), and skipped whole or part of year. Ungraded, partial credit and no information were considered missing. The composite variable of nonstandard progression included assignment to special education classes as defined above for special education assignment, partial credit for year, retained for all or part of year, assignment to alternative education facility, assignment to correctional facility, and institutionalized care. Standard progression included the categories of regular and social promotion, skipped, gifted classes, speech and/or hearing classes, and classes for the handicapped (orthopedically or perceptually).

Notes on the Data. The data shown in the tables and figures are preliminary in several instances and are not entirely comparable to the data presented in earlier reports. Although the source for most of the data was the 1980 School Data Form (Part A) obtained from school records by the staffs of the individual Consortium members, there are some important exceptions to be noted. For the Beller, Gray, and Miller projects, the 1980 School Data Form is the source of the data, data collection is virtually complete, and the 1980 data is in essential agreement with earlier data.

For the Gordon/Jester project, the 1980 data collection was not complete by the date of this report and so data by grade through the 4th grade sent to us prior to the 1976-1977 Follow-up was used instead. It is virtually the same data used in earlier reports. Because this year the

Karnes project was able to constitute a control group for the Five Approaches program groups, the program/control comparison had never been analyzed previously. The Karnes data came from the 1980 School Data Form and data collection is substantially completed.

The Levenstein project collected data for a different control group than was used previously in the school competence analyses: the after-only control group (group 19) which does not have a preprogram IQ score. Of the program groups, 1980 data were collected exclusively from the 1968 program group who had had the full two years of the program (T68 I&II, group 5). For the Palmer project, we have omitted the data on special education assignment because the data collected on this variable was based primarily on information from parent interviews, rather than school records.

The data from Weikart's Perry Preschool program were not received in time to be included in the tables and figures reported here, but will appear in later reports. For the present analyses, we used the same data as in 1979; these data were published in a monograph of the High/Scope Foundation. (See Weber, Foster, Weikart, 1978)

Groups Included. As in our previous analyses, we have excluded certain groups based on our desire to use the most rigorous comparisons. The groups were essentially identical to our previous analyses. For Beller's project we excluded the First Grade only group (group 3); for Gordon/Jester's project we excluded the subjects outside the main county school district; for Gray's project we excluded the Distal Control group (group 4); for Karnes we included the Five Approaches program groups (1-7, 10) and the most rigorously defined controls in waves one and two (that is, those subjects who did not participate in the program because of "space limitations"). For Levenstein's project we included subjects in the two year treatment group (T68 I&II, group 5) and the After-only Control subjects (group 19); for Miller's project we excluded Post-hoc controls (groups 7 and 8); and for Palmer's and Weikart's projects, no groups were excluded.

Results: Descriptive Tables and Figures

The data from eight projects for the 3 dependent variables are summarized in Tables 14, 15, 16 and shown in Figures 1-3. For special education assignment, the figures for cumulative percent ever assigned to special education by grade (Figur show that early education results in a reduction in rates of assignment. This is as previously reported. With the data by grade we can now begin to determine the timing of the differential effect of early education, a step beyond our earlier results. In general, the impact of early education appears after several grades rather than immediately. Because each project is located in completely different school districts each with different policies and resources for assignment to special education classes, it is not surprising that the grade at which the rate of assignment of program and control children begins to diverge varies with the project. For the Beller, Gordon/Jester, and Weikart projects, the effect begins to appear in the 2nd grade; in the 4th grade for Gray; and the 6th grade for Karnes. However, for Levenstein, the program subjects had higher rates of assignment until the 8th grade when the rates converge. For Miller's project the program children have higher rates of assignment than the controls and for Beller's project the rates for the program and controls converge by the 5th grade and by the 9th grade the controls have a slightly lower rate of assignment than the programs.

For retention in grade, Figure 2 of cumulative percent ever retained by grade show that early education results in a reduction of rates of retention for six of eight projects: for the Beller project starting in third grade; the Karnes project starting in Kindergarten; the Levenstein project starting in first grade; the Miller project starting in eighth grade' the Palmer project starting in third grade; and for the Weikart project the rate of retention is slightly lower for the program group through fourth grade, but in fifth grade the cumulative rates are about equal. However, for the Gordon/Jester and Gray projects the rates are about equal with a very slight tendency for the program groups to have a higher rate or retention.

For nonstandard progression (the composite variable) the Figure 3 shows that early education results in reduced rates of assignment or seven of eight projects: for Beller starting in the third grade and continuing through the tenth grade; for Gordon/Jester starting in second grade; for Karnes starting in kindergarten; for Levenstein starting in seventh grade; for Palmer starting in third grade; and for Weikart starting in second grade. However, for Miller, the program subjects have higher rates than the controls, although the rates converge by about seventh grade.

This year was meant to be a data collection year, not an anaylsis year. Nevertheless, some preliminary results of analysis are reported here. In general, the results are similar to those reported last year. In the coming year, we plan to finish the data collection and editing process and analyze the school career data to provide insights into the timing and sequence of important direct measures of school competence, such as special education assignment, grade retention, and the composite variable of nonstandard progression.

Table 14

Cumulative Percent Ever Assigned to Special Education by Given Grade

	Beller		Gordon/ Jester		Gray		Karnes		Leven		Miller		Weikart	
Grade	C[a]	P	C	P	C	P	C	P	C	P	C	P	C	P
Kinder.	0	0	0	0	0	0	0	0	0	0	0	0	0	0
1	0	0	16.7	11.9	0	0	7.7	8.0	0	0	0	0	0	0
2	0	0	41.7	19.4	0	0	11.5	10.2	3.8	5.6	0	0	7.7	5.2
3	5.4	0	50.0	20.9	0	0	15.4	18.2	3.8	11.1	0	0	20.0	10.3
4	5.4	0	50.0	23.9	9.5	2.4	19.2	18.2	3.8	16.7	0	1.1	27.7	15.5
5	5.4	4.5	--	--	9.5	2.4	19.2	18.2	11.5	16.7	0	2.3	30.8	15.5
6	5.4	4.5	--	--	23.8	2.4	23.1	18.2	11.5	22.2	0	9.2	--	--
7	5.4	4.5	--	--	23.8	2.4	26.9	21.6	19.2	22.2	5.9	13.8	--	--
8	5.4	4.5	--	--	23.8	2.4	26.9	21.6	23.1	22.2	5.9	16.1	--	--
9	5.4	9.1	--	--	23.8	2.4	26.9	22.7	--	--	5.9	17.2	--	--
10	5.4	9.1	--	--	23.8	4.9	42.3	26.1	--	--	--	--	--	--
11	5.4	9.1	--	--	28.6	4.9	46.2	30.7	--	--	--	--	--	--
12	5.4	9.1	--	--	28.6	7.3	50.0	36.4	--	--	--	--	--	--
Never Assigned	94.6	90.9	50.0	76.1	71.4	92.7	50.0	63.6	76.9	77.8	94.1	82.8	64.6	81.0
N	37	44	12	67	21	41	26	88	26	18	17	87	65	58

[a] C= Control subjects
P= Program subjects

Table 15

Cumulative Percent Ever Retained by Given Grade

	Beller		Gordon/ Jester		Gray		Karnes		Leven		Miller		Palmer		Weikart	
Grade	C[a]	P	C	P	C	P	C	P	C	P	C	P	C	P	C	P
Kinder.	0	2.3	0	4.5	0	0	3.8	1.1	0	0	0	0	0	0	0	0
1	0	2.3	0	7.5	14.3	19.5	7.7	4.5	3.8	0	0	4.6	0	0	0	0
2	2.7	2.3	0	7.5	23.8	26.8	7.7	5.7	3.8	0	0	4.6	0	0	4.6	1.7
3	5.4	2.3	8.3	9.0	28.6	34.1	11.5	8.0	3.8	0	0	4.6	11.8	3.6	6.2	3.4
4	8.1	6.8	8.3	10.4	38.1	41.5	11.5	8.0	3.8	0	0	4.6	21.6	6.1	10.8	6.9
5	10.8	6.8	--	--	42.9	43.9	11.5	8.0	3.8	0	0	6.9	35.3	14.5	10.8	12.1
6	13.5	9.1	--	--	47.6	43.9	11.5	8.0	3.8	0	0	6.9	45.1	23.0	--	--
7	13.5	9.1	--	--	47.6	46.3	11.5	9.1	11.5	5.6	11.8	12.6	51.0	29.7	--	--
8	18.9	11.4	--	--	52.4	46.3	11.5	9.1	11.5	5.6	23.5	21.8	51.0	30.3	--	--
9	29.7	13.6	--	--	52.4	51.2	11.5	11.4	--	--	23.5	21.8	--	--	--	--
10	45.9	27.3	--	--	52.4	56.1	11.5	11.4	--	--	--	--	--	--	--	--
11	48.6	34.1	--	--	57.1	56.1	11.5	13.6	--	--	--	--	--	--	--	--
12	51.4	36.4	--	--	57.1	56.1	15.4	15.9	--	--	--	--	--	--	--	--
Never Assigned	48.6	63.6	91.7	89.6	42.9	43.9	84.6	84.1	88.5	94.4	76.5	78.2	23.6	76.4	87.7	81.0
N	37	44	12	67	21	41	26	88	26	18	17	87	51	165	65	58

[a] C = Control subjects
P = Program subjects

Table 16

Cumulative Percent Ever Off Normal Progression by Given Grade

	Beller		Gordon/ Jester		Gray		Karnes		Leven		Miller		Palmer		Weikart	
Grade	C[a]	P	C	P	C	P	C	P	C	P	C	P	C	P	C	P
Kinder.	0	2.3	0	4.5	0	0	3.8	1.1	0	0	0	0	0	0	0	0
1	0	2.3	16.7	17.9	14.3	19.5	15.4	12.5	3.8	0	0	4.6	2.0	1.8	0	0
2	2.7	2.3	41.7	25.4	23.8	26.8	19.2	13.6	7.7	5.6	0	4.6	3.9	1.8	12.3	6.9
3	8.1	2.3	50.0	28.4	28.6	34.1	23.1	20.5	7.7	11.1	0	4.6	15.7	6.1	26.2	12.1
4	10.8	6.8	50.0	31.3	42.9	43.9	26.9	20.5	7.7	16.7	0	5.7	25.5	9.7	36.9	19.0
5	13.5	9.1	--	--	47.6	46.3	26.9	20.5	15.4	16.7	0	8.0	39.2	18.2	40.0	24.1
6	16.2	11.4	--	--	61.9	46.3	30.8	20.5	15.4	22.2	0	13.8	47.1	26.7	--	--
7	16.2	11.4	--	--	61.9	48.8	34.6	25.0	30.8	22.2	23.5	27.6	52.9	35.2	--	--
8	21.6	13.6	--	--	66.7	48.8	34.6	25.0	30.8	22.2	35.3	35.6	54.9	35.8	--	--
9	32.4	25.0	--	--	66.7	53.7	34.6	27.3	--	--	35.3	35.6	--	--	--	--
10	48.6	43.2	--	--	71.4	58.5	50.0	33.0	--	--	--	--	--	--	--	--
11	51.4	52.3	--	--	71.4	61.0	53.8	39.8	--	--	--	--	--	--	--	--
12	54.1	54.5	--	--	71.4	61.0	57.7	46.6	--	--	--	--	--	--	--	--
Never Assigned	45.9	45.5	50.0	68.7	28.6	39.0	42.3	53.4	69.2	77.8	64.7	64.4	45.1	64.2	55.4	70.7
N	37	44	12	67	21	41	26	88	26	18	17	87	51	165	65	58

[a] C = Control subjects
P = Program subjects

Figure 1

Cumulative Percent Ever Assigned to Special Education by Given Grade

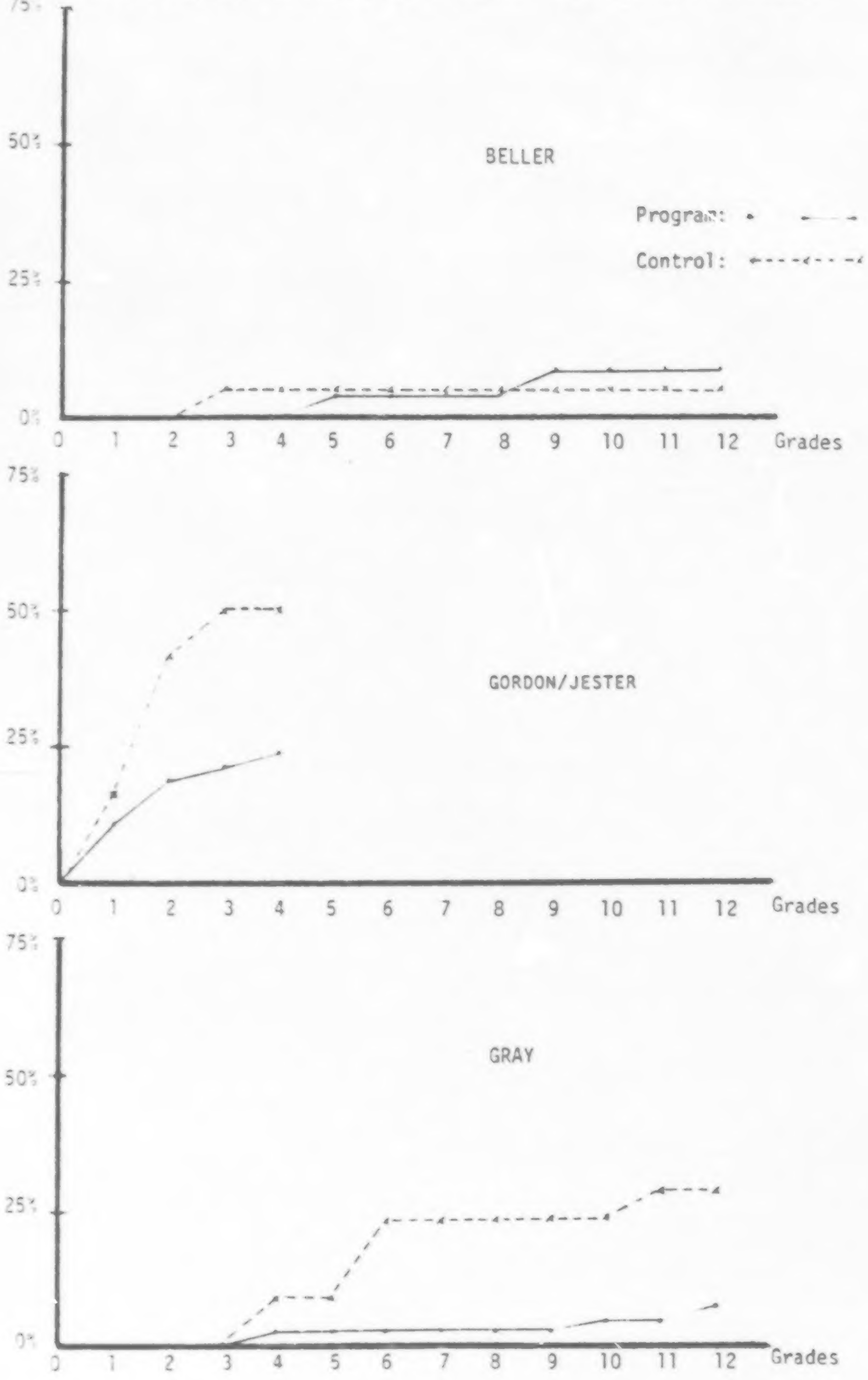


Figure 1 (Cont.)

Cumulative Percent Ever Assigned to Special Education by Given Grade

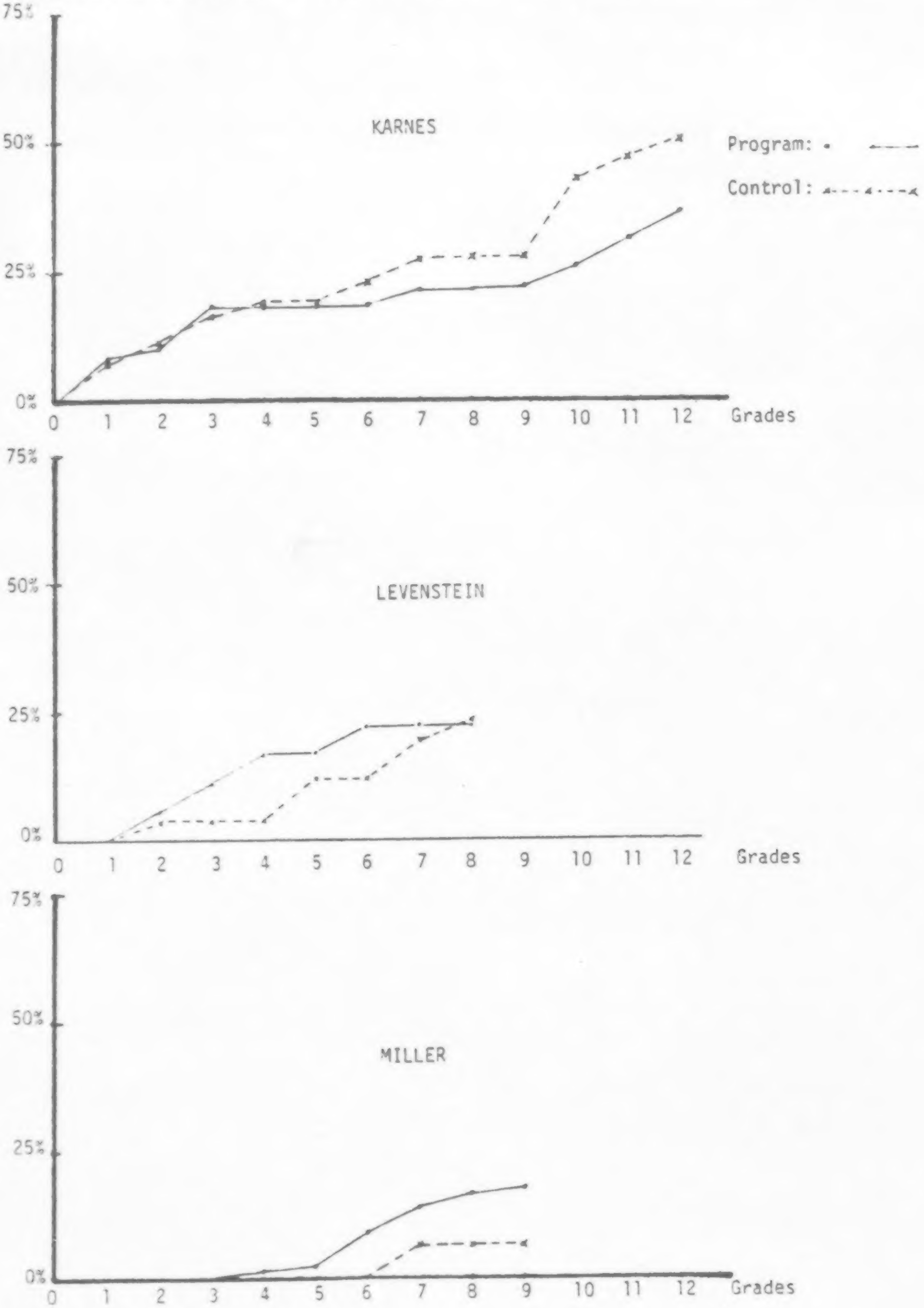


Figure 1 (cont.)

Cumulative Percent Ever Assigned to Special Education by Given Grade

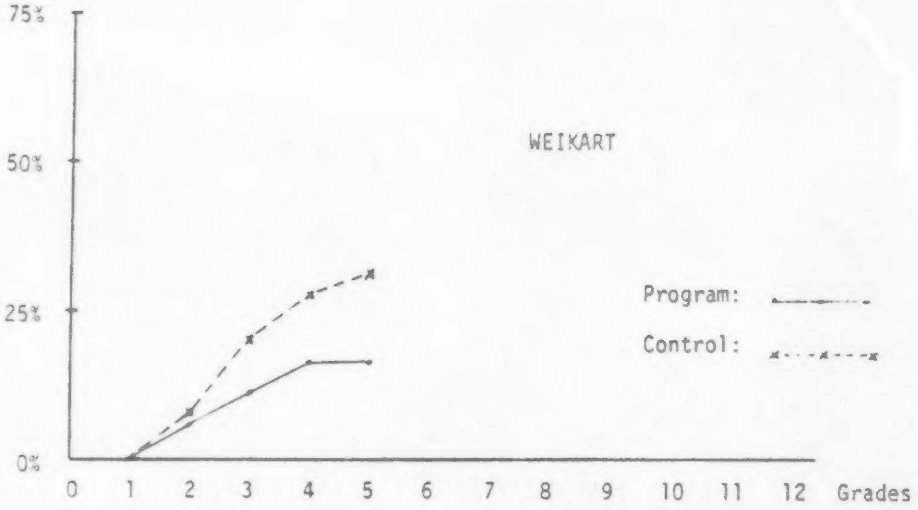


Figure 2

Cumulative Percent Ever Retained by Given Grade

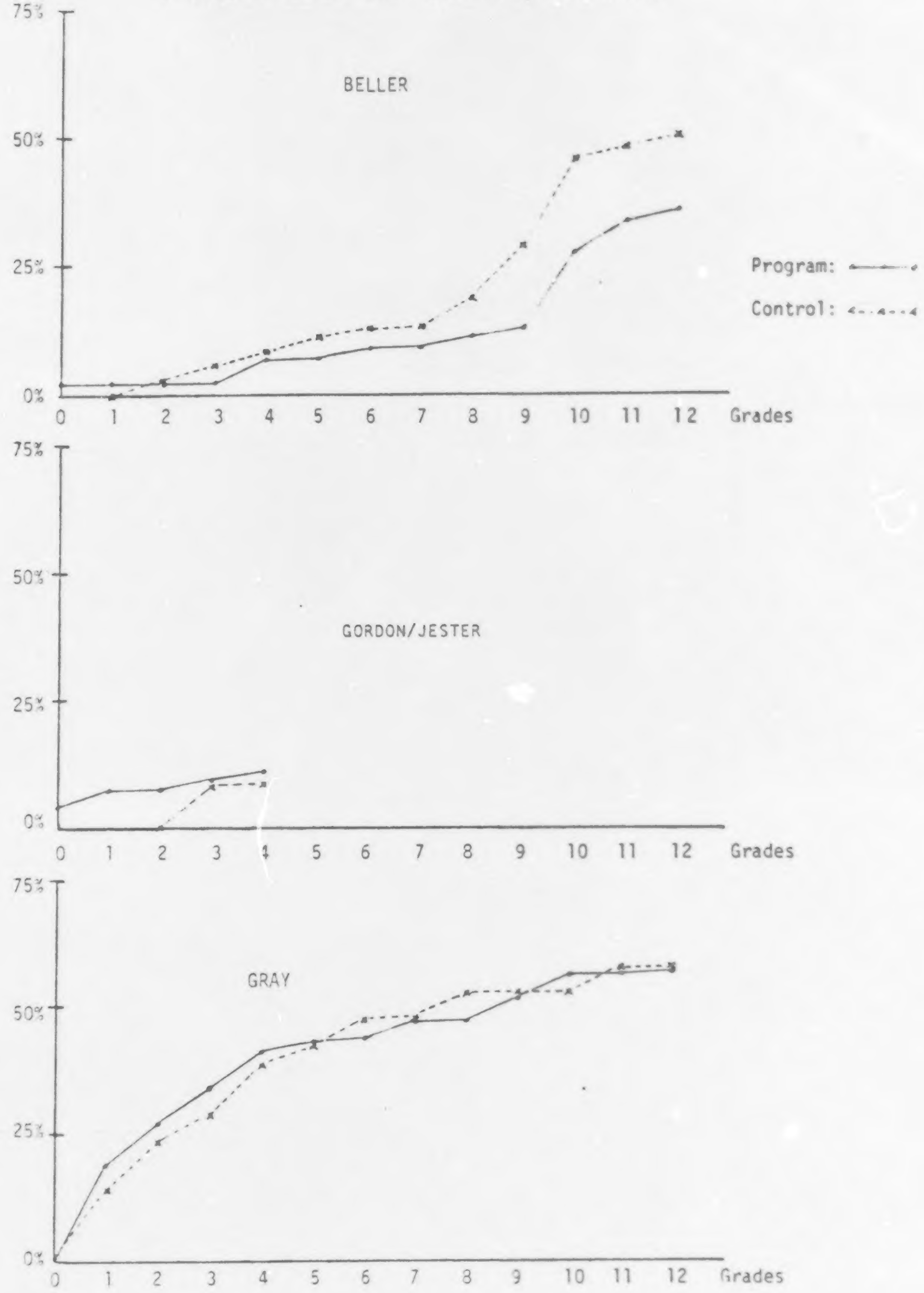


Figure 2 (cont.)

Cumulative Percent Ever Retained by Given Grade

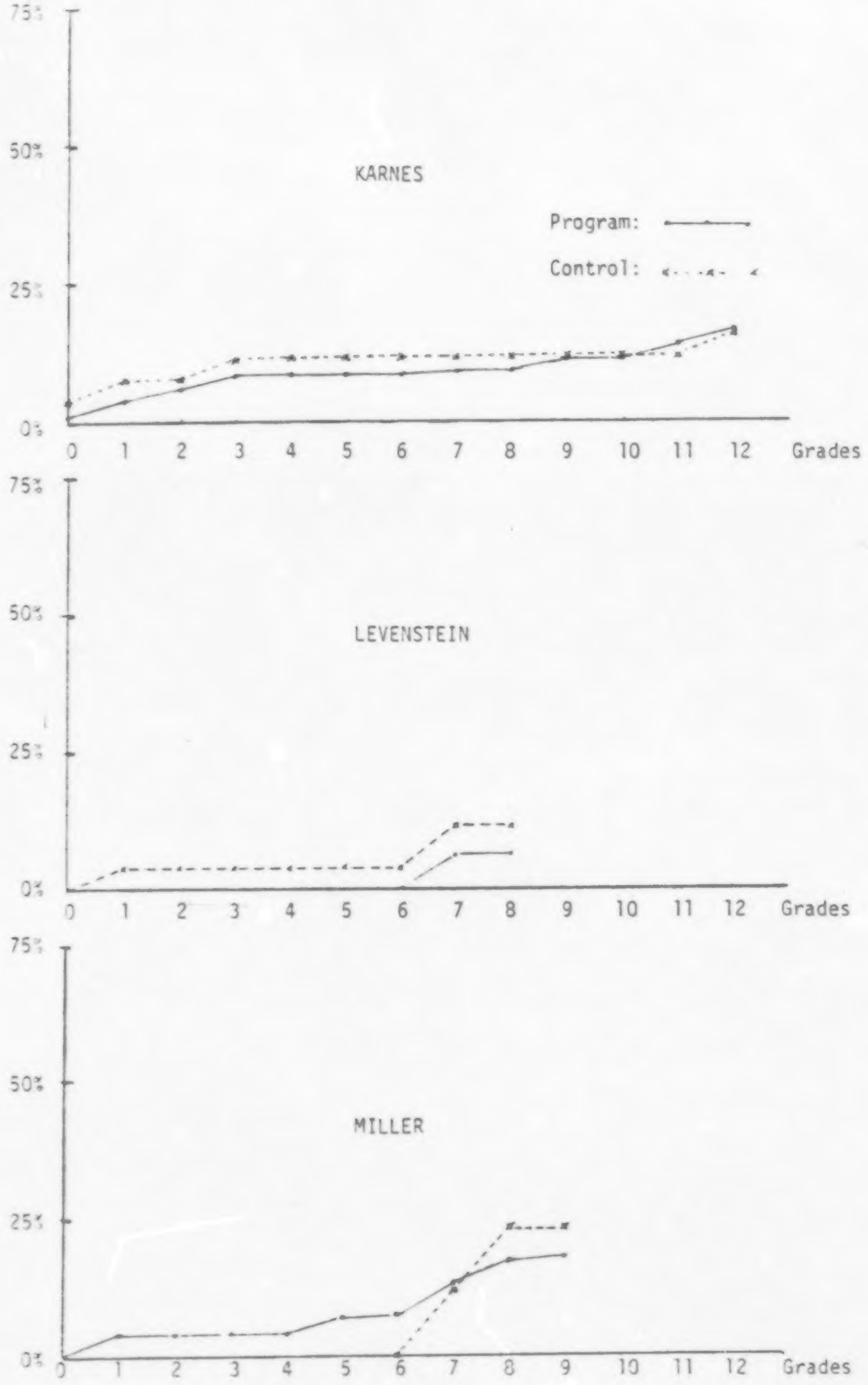


Figure 2 (cont.)

Cumulative Percent Ever Retained by Given Grade

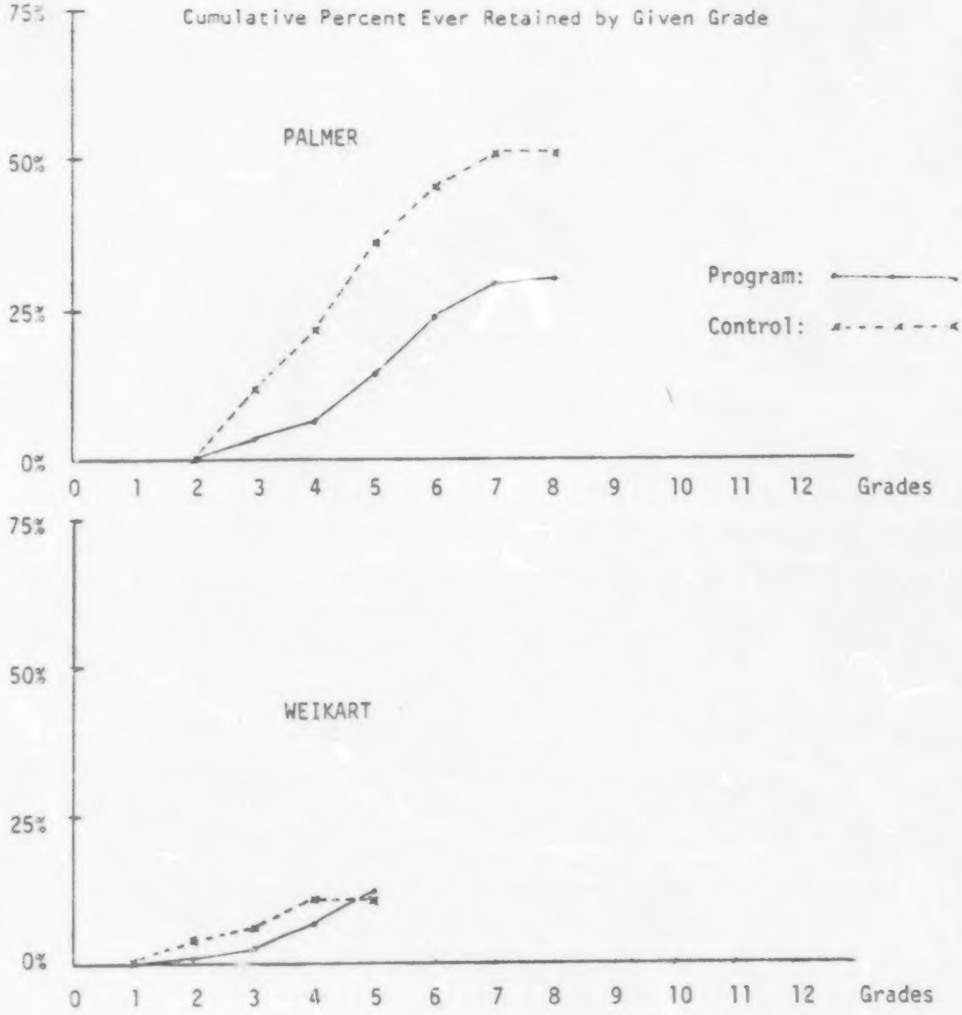


Figure 3

Cumulative Percent Ever Off Normal Progression by Given Grade

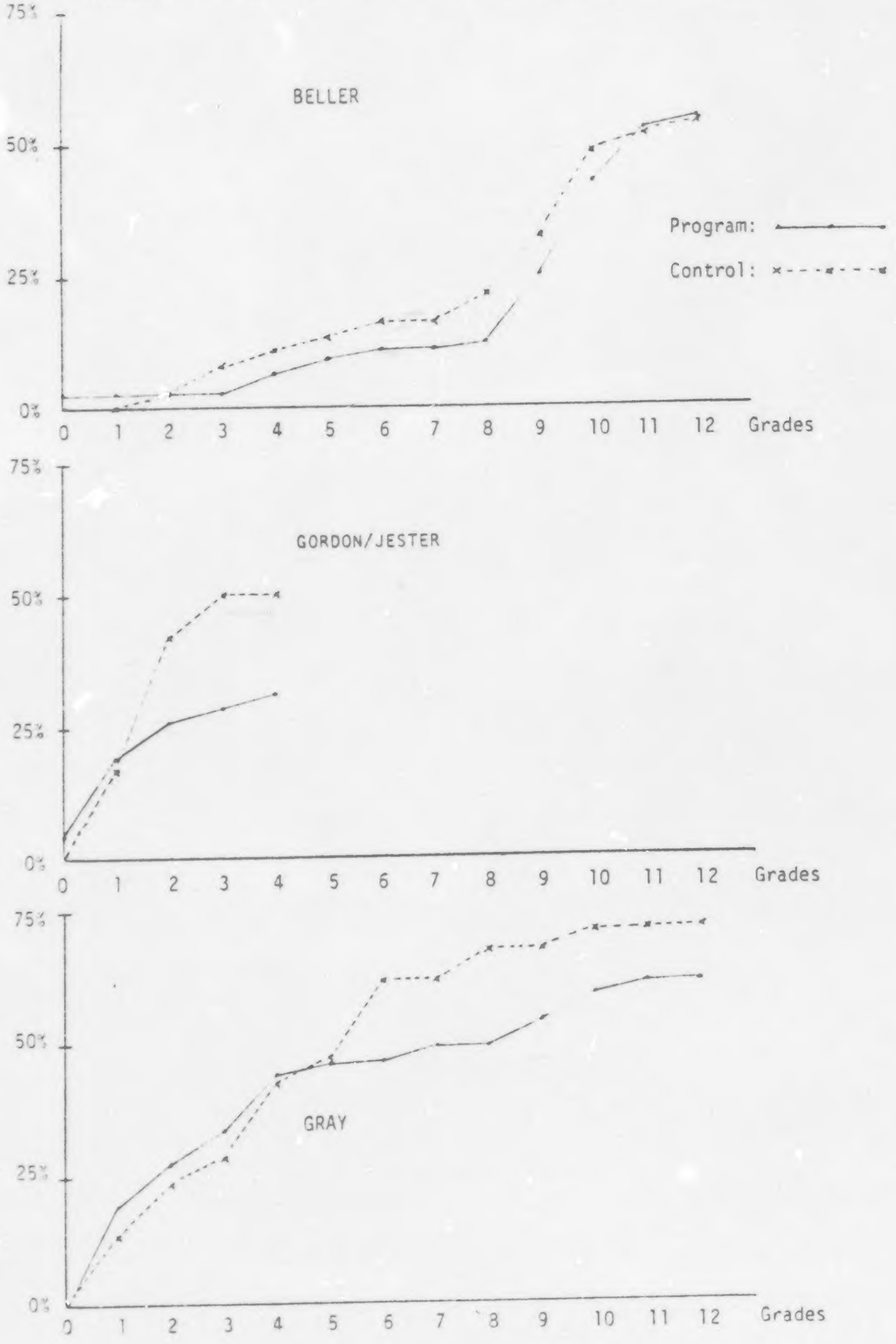


Figure 3 (cont.)

Cumulative Percent Ever Off Normal Progression by Given Grade

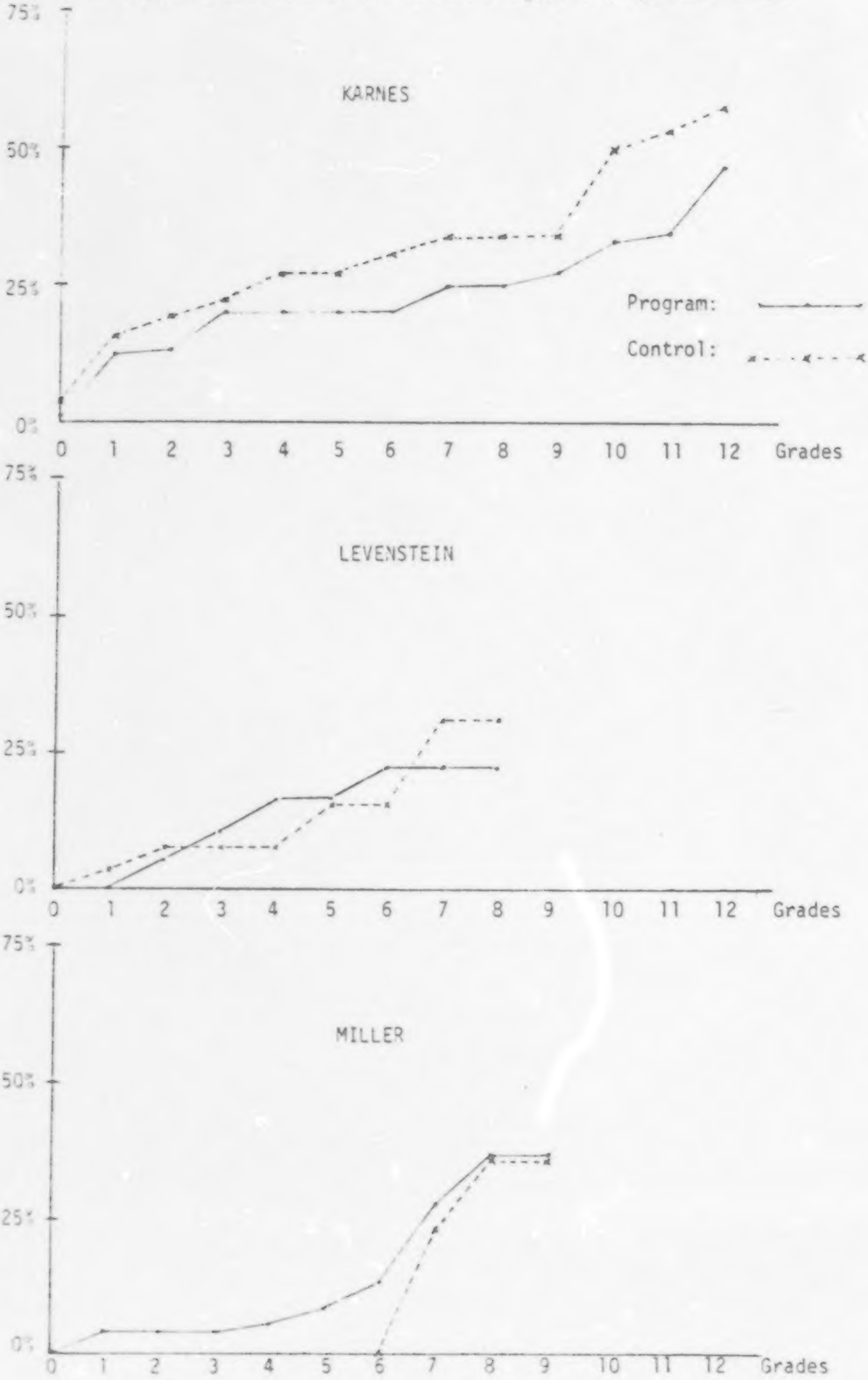
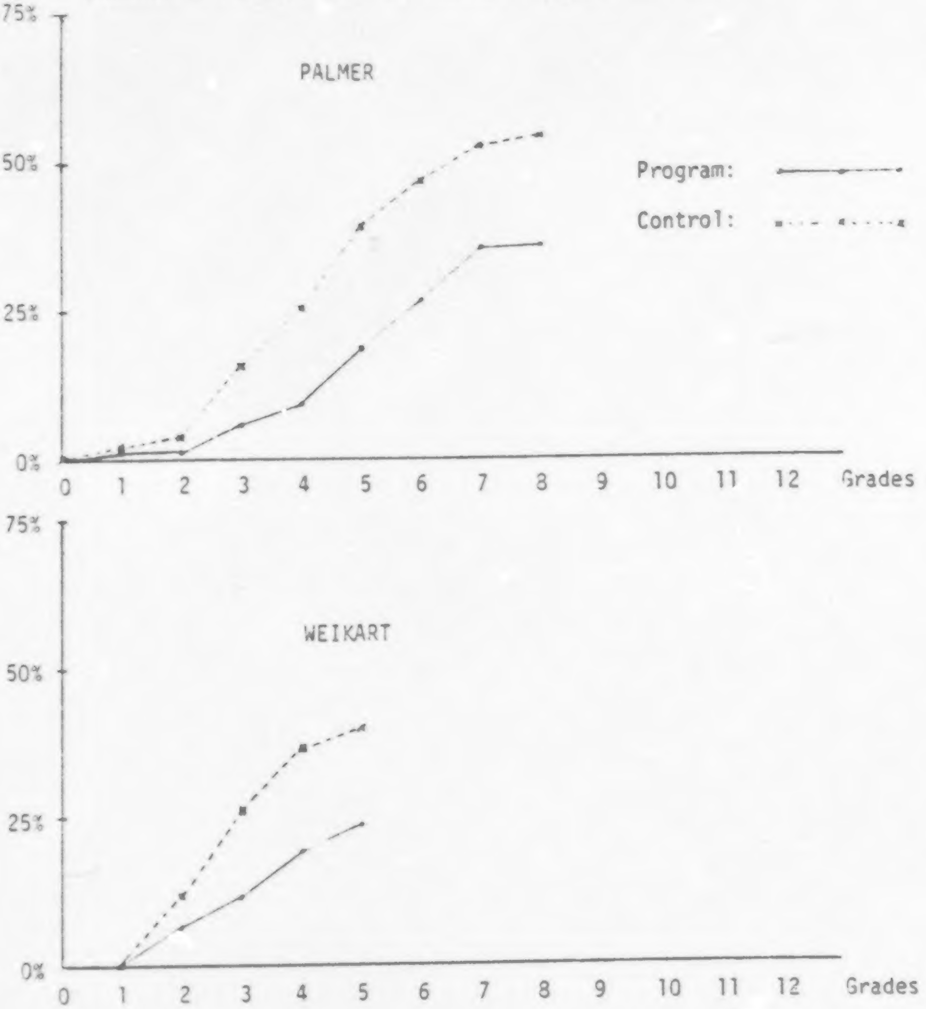


Figure 3 (cont.)

Cumulative Percent Ever Off Normal Progression by Given Grade



Effect of Early Education on School Competence: Tracking

Students in secondary school are inevitably faced with post high school career decisions. Theoretically, school systems recognize this by providing students with the opportunity to choose from a variety of academic tracks concomitant with their career interests. Qualified students who wish to enter college have a college preparatory (PREP) curriculum available to them which provides the necessary course background for later academic success in college. Students wishing to enter the job market directly after high school are provided opportunities for vocational or technical training (VOC). These programs offer work experience and may enhance the students' marketability in the job world directly after high school. Finally, students who are either undecided about a career prep or vocational track can select for themselves a regular or general (REG) high school track.

Clearly, the tracking system described above represents an ideal which is rarely achieved in practice. More realistically, student track placement is a complex function of interacting variables. Peer and parental influence, together with educational assessment outcomes, school policies and resources, as well as actual student ability and motivational factors, are major determinants of high school track. Obviously, this kind of reasoning prevents one from assuming a simple relationship between student ability and high school track placement. One might reasonably expect that participation in early education programs could indirectly affect high school track decisions by first influencing child motivational factors, which in turn might affect teacher and parental relationships. Obviously, the indirect effects postulated here suggest a rather complex model specifying a number of subtle yet important interrelationships between early educational programs and intervening school, peer, and family variables, which could finally affect high school track decisions. However, specification and testing of such a model was beyond the scope of this analysis.

The analysis reported here was preliminary in nature and looked only for a simple relationship between preschool attendance and later high school track placement. The specific hypothesis was a significant program-control difference in track assignment for students at grades 11 and 12 from the program-control (PC) variable, while holding constant the effects of mother education (MED), preschool IQ test scores (PREIQ), and subject sex (SEX). Small sample sizes made impractical the inclusion of all reasonable covariates.

Only data for the Beller and Gray projects were analyzed since insufficient data were available for the other projects. Also, the Beller and Gray projects were analyzed somewhat differently since the school system for the Beller project assigned high school students to one of three track categories (VOC, REG, PREP), while the school system for the Gray project utilized only two track categories (VOC, REG). Absence of the college preparatory category for Gray was apparently due to lack of differentiation of the regular track into the REG and PREP categories utilized by the Beller school system. Presumably, a special college preparatory track was not available to students from the Gray project.

The assumption of ordinality of track classification was explicitly rejected for this analysis. That is, college prep was not assumed to be the highest or most desirable track followed by the regular and finally the vocational tracks. See Table 17 for actual student frequencies in each track category, by project.

Specifically, for Beller, the analysis of track for both grades 11 and 12 was performed on the following dichotomous dependent measures: (1) VOC versus REG, (2) VOC versus PREP, (3) REG versus PREP, and (4) VOC or REG versus PREP. Since only two track categories were available for the Gray project at grades 11 and 12, the analysis at each grade was performed on the single dichotomous dependent variable VOC versus REG. A general specification of the variables in the regression model would be

$$\text{TRACK} = \text{MED} + \text{PREIQ} + \text{SEX} + \underline{\text{PC}},$$

where the predictor of interest is PC and the covariates are MED, PREIQ, and SEX. Separate regressions were run for grades 11 and 12. In each case the criterion "TRACK" was one of the above mentioned dichotomous dependent variables. Separate regressions were performed on the Beller and Gray projects.

For both the Beller and Gray projects, individual students were excluded from the analysis if they were ever assigned to any one of the following special education categories: unspecialized special education, educable mentally retarded, trainable mentally retarded, emotionally disturbed, or learning disability. Also excluded were children placed in correctional facilities or other special schools. In keeping with previous analyses, Beller's first grade control group was excluded; also Gray's distal control group was excluded from the analysis.

Occasionally students were held back in grades 11 or 12, creating the problem of more than one possible track assignment for a given grade. In such a case, the first track assignment for the failed grade was used as the representative track level for that grade irrespective of whether or not the student had changed track when repeating the grade. Finally, students who were assigned to a particular track but quit school in either grade 11 or 12 (after having formally enrolled for that year) were included in the analysis for that grade.

Results indicated no simple chi-square relationship between the program control variable and the various dichotomous decompositions of track assignment in either grade 11 or grade 12 (see Table 18).

Multiple regression results were also nonsignificant. Although sample sizes were small, and consequently tests of significance lacked power, there was no evidence of program-control effects of track assignment in either grade 11 or grade 12, when controlling for sex, pretest IQ, and mothers' education. (See Table 19). Trends in regression coefficients for program-control were also lacking. In short, one observes in the sample essentially no relationship between early education and high school track assignment for grades 11 and 12. It should be emphasized, however, that the present analysis is only preliminary and will be repeated as more data becomes available, using a model which will attempt to more explicitly specify intervening causual relationships.

Table 17

Number of Subjects in Each Track Assignment Category for Grades 11 and 12, by Project

Track Assignment	GRADE 11		GRADE 12	
	Program	Control	Program	Control
Beller Project				
VOC	9	4	9	4
REG	15	11	12	11
PREP	7	7	8	7
Total	31	22	29	22
Gray Project				
VOC	7	3	6	3
REG	17	6	15	6
Total	24	9	21	9

Table 18

Effect of Early Education on Track Assignment in 11th and 12th Grades: Program vs. Control by Track Assignment

Track Assignment	GRADE 11 N	GRADE 11 Chi-Square	GRADE 11 p(2-tail)	GRADE 12 N	GRADE 12 Chi-Square	GRADE 12 p(2-tail)
Beller Project						
VOC vs. REG	39	0.49	.485	36	0.99	.319
VOC vs. PREP	27	1.03	.310	28	0.74	.390
REG vs. PREP	40	0.22	.641	38	0.01	.944
REG or VOC vs. PREP	53	0.56	.452	51	0.11	.742
Gray Project						
VOC vs. REG	33	0.05	.817	30	0.07	.794

Note. Chi-Square is uncorrected (Camilli and Hopkins, 1978). Track assignments are: vocational, regular, or college preparatory. All track assignment contrasts were dichotomized.

Table 19

Effect of Early Education on Track Assignment at 11th and 12th Grades When Background Variables and Pretest IQ are Controlled

Track Assignment	GRADE 11 N	GRADE 11 b	GRADE 11 t	GRADE 12 N	GRADE 12 b	GRADE 12 t
Beller Project						
VOC vs. REG	28	.035	0.148	28	-.118	-0.560
VOC vs. PREP	23	-.004	-0.010	24	-.015	-0.063
REG vs. PREP	29	-.049	-0.217	30	.124	0.604
REG or VOC vs. PREP	40	-.037	-0.210	41	.038	0.230
Gray Project						
VOC vs. REG	33	.096	0.533	30	.068	0.369

Note. b is unstandardized regression coefficient for the program-control variable. Negative coefficient indicates control subjects were "higher" on the track assignment variable. Equation: TRACK = MED + PREIQ + SEX + PC; see text for legend. None of the regression coefficients were significant.

Effect of Early Education on School Competence: Course Marks

The most traditional and perhaps most direct index of academic performance is course marks. Academic performance, however is usually considered to be some function of both student ability and motivation (Weiner, 1974). That is, when ability is held constant, academic performance or, more specifically, course marks might be favorably influenced by any intervention that succeeded in increasing student motivation to learn.

In its philosophy, early childhood education is often regarded as an attempt to increase child academic performance by influencing both ability and motivational factors. On the surface, the motivational factor is more readily seen to be influenced by the early education experience than the ability component. However, developmental theorists are becoming increasingly aware that the component of academic ability is not a simple psychological given (Zigler and Trickett, 1978). That is, academic ability may be further decomposed into two factors: (1) native aptitude, and (2) a more fluid component of conceptual learning ability related to level of cognitive readiness or development. Although early education programs may vary considerably in terms of relative emphases, almost all attempt to facilitate academic performance by increasing child motivation and at the same time fostering growth in cognitive development.

Clearly then, one might theoretically argue that early education should produce a favorable imapct on academic performance. In fact, previous research (Consortium, 1978) on child developed abilities has indicated that children who attended early education programs surpassed their controls on the Stanford-Binet intelligence test for several years after the program had ended. Also, there was some evidence that early education temporarily improved achievement test scores. Given these positive findings concerning child developed abilities, it seems reasonable to postulate a favorable relationship between early education and other more widely accepted measures of academic success, such as course marks. Specifically, it was hypothesized in the present analysis that a variable representing program experience versus no program (PC) would be a significant predictor of course marks. Grade levels 4,6,and 10 were selected as criterion grades since they were fairly indicative of transition years and sample sizes were generally adequate. At each grade level PC effects were evaluated for (a) overall GPA, (b) reading grades, and (c) math grades. The possible confounding effects of sex (SEX), program I.Q. scores (PREIQ), and mother education (MED), were controlled by ordinary least squares regression techniques. When sample size was sufficient, the variables in the regression model were as follows.

COURSE MARK = SEX + PREIQ + MED + PC,

where the dependent variable was one of nine combinations of grade level and specific type of course mark; the covariates were SEX, PREIQ, and MED; the predictor of interest was PC.

Data was analyzed individually for four projects: Beller, Gray, Karnes, and Miller. However, a sufficiently large number of subjects was not available for grades 4,6, and 10 from each project. Consequently, only the Beller and Miller projects were used on fourth grade course marks. All four projects had a sufficiently large N for sixth grade analysis. The Beller, Gray, and Karnes projects were used in the tenth grade analysis.

The range of scores on each dependent variable was from one (failure) to five (excellent). In a small number of cases grades were merely reported as "satisfactory" or "unsatisfactory". These were respectively recoded to values of three (average) or one (failure) on the above scale.

For all the projects, individual students were excluded from the analysis if assigned to any one of the following special education categories: unspecialized special education, educable mentally retarded, trainable mentally retarded, emotionally disturbed, or learning disability. Also excluded were children placed in correctional facilities or other special schools. In keeping with previous analyses, Beller's first grade control group, as well as Gray's distal control group, were excluded from the analysis; also, all of Karnes' originally specified control subjects were used.

Occasionally students were held back in the grades of interest, creating the problem of more than one possible course mark reported for a given grade level. In such as case, the initial course mark reported for that grade level was used in the analysis.

Results from the analyses indicated no consistent simple chi-square relationship between the program-control variable and course marks (see Table 20).

Furthermore, pooled _z_'s from multiple regression analysis revealed no significant (2-tailed) early education effects on course marks at grade levels 4,6, or 10. (See Table 21). However, a nonsignificant trend in regression weights may be observed for early education at grade four (See Table 21). Note also that early education is two-tailed significant at p = .053 for reading course marks at grade four.

In generalizing from these samples, it would appear that there is no strong relationship between the preschool experience and academic performance as measured by course marks. However, it should also be noted that the present analysis made no attempt to test for possible interactions between ability and motivation. Certainly, one could argue that preschool attendance primarily affects the child's motivation to learn, and that motivation interacts with native ability to produce differential effects on academic performance. In such a case, it would not be reasonable to expect a significant regression weight for PC. However, one could expect a significant regression weight for a variable representing the interaction between early education and child I.Q. This hypothesis is to be tested in subsequent analyses.

Table 20

Effect of Early Education on Course Marks (Overall GPA, Reading, Math) in Grades Four, Six, and Ten

Type of Grade	GRADE 4 N	GRADE 4 Chi-Square	GRADE 4 p (2-tail)	GRADE 6 N	GRADE 6 Chi-Square	GRADE 6 p (2-tail)	GRADE 10 N	GRADE 10 Chi-Square	GRADE 10 p (2-tail)
Overall									
Beller	55	1.29	.863	58	4.89	.429	53	2.88	.577
Gray				38	0.43	.934	28	3.93	.416
Karnes				20	2.71	.258	104	7.69	.104
Miller	67	2.86	.414	81	0.90	.924			
Reading									
Beller	57	1.68	.642	55	1.38	.848	52	5.49	.240
Gray				38	0.48	.923	34	4.92	.296
Karnes				20	---[1]	---	96	7.68	.104
Miller	65	5.69	.128	79	3.33	.504			
Math									
Beller	56	10.43	.034*	54	6.01	.198	53	4.83	.305
Gray				36	9.06	.060	13	0.87	.832
Karnes				20	0.25	.883	84	1.55	.818
Miller	63	8.46	.076	79	2.56	.633			

* Significant at .05 level.

[1] Too many zero frequency cells to calculate Chi-Square (Chi-Square is uncorrected: Camilli and Hopkins, 1978).

Table 21

Effect of Early Education on Course Marks (Overall GPA, Reading, Math) in Grades Four, Six, and Ten When Background Variables and Pretest IQ are Controlled

Type of Grade	GRADE 4 N	GRADE 4 b	GRADE 4 t	GRADE 4 pooled z	GRADE 4 (2-tail) pooled p	GRADE 6 N	GRADE 6 b	GRADE 6 t	GRADE 6 pooled z	GRADE 6 (2-tail) pooled p	GRADE 10 N	GRADE 10 b	GRADE 10 t	GRADE 10 pooled z	GRADE 10 (2-tail) pooled p
Overall															
Beller	40	.290	0.953			45	.495	1.423			41	.758	1.996		
Gray						38	.034	0.173			28[a]	-.340	-0.689		
Karnes						20[a]	-.179	-0.680			99[b]	.242	0.126		
Miller	65	.252	1.009	1.373	.170	79	-.244	-1.040	-0.064	.949				0.796	.426
Reading															
Beller	41	.418	1.365			42	.397	1.358			38	.565	1.174		
Gray						38	-.109	-0.471			34	-.480	-1.153		
Karnes						20[a]	.060	0.324			93[b]	-.346	-1.479		
Miller	63	.391	1.414	1.933	.053	77	-.260	-0.965	0.112	.910				-0.832	.405
Math															
Beller	40	-.262	-0.665			41	.200	0.620			38	.485	0.986		
Gray						36	-.317	-1.185			13[c]	-.322	-0.336		
Karnes						20[a]	-.094	-0.288			80[b]	-.256	-0.887		
Miller	62	.605	1.907	0.857	.392	77	-.076	-0.251	-0.541	.588				-0.137	.891

Note. b is unstandardized regression coefficient for the program-control variable. Negative coefficient indicates control subjects obtained higher grades. Except when otherwise noted, the regression equation was: GRADE = MED + PREIQ + SEX + PC; see text for legend.

[a] Because of restricted sample size, only SEX and PREIQ were used as covariates.
[b] RACE was included as an additional covariate; this variable was not available for the other projects.
[c] Because of very small sample size, only PREIQ was used as a covariate.

PLANS FOR FY1981

Data Analysis FY1981

Our FY1981 analyses will continue to focus on the question of the effectiveness of early education -- both its "average" effect on outcomes and its differential effect on outcomes for different types of children. The FY1981 analyses will extend our previous analyses in three ways: first, by exploring the effect of early education on additional outcomes; second, by utilizing our longitudinal school record data and our 1980 interview data to explore relationships between early and later outcomes; and third, by incorporating new background independent variables, both as covariates and as possible factors in differential effectiveness. More specific plans for these three extensions of our earlier work are discussed below.

The FY1981 outcome measures will be of two kinds: educational outcomes and labor market outcomes. The educational outcomes, on which preliminary work is underway, consist of events in a youth's educational career obtained from school records: high school completion versus dropout (for older subjects), high school track, and suspensions. Data on suspensions and high school completion will be checked with information from youth interviews. These are ecologically valid measures similar to special education assignment and grade retention measures used in our previous analyses.

Labor market outcomes will also be investigated. Specification of the variables is more difficult for labor market outcomes than for educational outcomes. Experiences in the labor market are more varied and, hence, more difficult to summarize than educational outcomes. Furthermore, the specific labor markets which we propose to study -- those for low SES black youth -- differ in important ways from the labor market as a whole and a standard set of outcome indicators specifically designed for these labor markets has never been developed. Traditional indicators of labor market success are valuable but can be difficult to interpret. The often temporary nature of employment of low SES youth makes interpretation of wage and salary data difficult. Occupational prestige rankings often do not discriminate adequately at the lower end of the scale.

Given the difficulties of defining labor market success for this population, we have decided to employ several indicators, including both traditional labor market indicators and indicators which we will develop on the basis of contingency table and correlational analyses of the labor market and life style questions in the 1979 interview. Traditional indicators will include current employment status, wages, monthly earnings, prestige rating of current or last job, and job satisfaction. Labor force participation may be included as an indicator.

The second extension of our previous analyses will be the establishment of a chronological pattern of the effects of early education on educational outcomes. For instance, assignment to special education and retention in grade will be analyzed for each year in order to ascertain the chronological pattern of the already documented effects of early education on assignment to special education. Other analyses will treat earlier measures as intervening variables through which the effects of early education may be transmitted to later outcomes. For instance, we will investigate the effects of early education on high school completion as transmitted through its effect on retention-in-grade.

The third extension of our previous analyses will be the inclusion of additional family variables. These variables will be family background and process variables derived from the 1976 interview data. For example, our previous background demographic variables were derived from interviews at entry into the early education programs. We believe that the use of more recent demographic information may increase the explanatory power of the covariates as a whole and thus increase the power of the analysis.

Dissemination FY1981

The major dissemination effort for FY1981 will be writing a volume in collaboration with the Consortium members. A sustained, book-length treatment of the research is merited by the unique longitudinal nature of the data, the scope, both in breadth and depth, of the program evaluations, and the importance of the findings for both theoretical and practical questions. A book will serve to bring all the results together in one place, placing the research in historical and social context and illustrating the advantages and difficulties of collaborative research. It will include descriptions of both individual projects and the joint analyses that have been performed in Ithaca. The major thrust of the section on joint analyses will be to integrate the findings of the two follow-ups (1976-77 and 1979-80) in an attempt to understand the scope and the mechanisms of preschools' effect on children from low-income families. As such, it will summarize and evaluate our work over the past five years.

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ATTACHMENT A

ATTRITION IN THE 1980 FOLLOW-UP SAMPLE

The attrition analyses reported here are based on data received as of September 30, 1980. For some projects, 1979-80 data collection was essentially completed (Beller, Gray, Levenstein, Palmer); for others data collection is continuing for various reasons (Gordon/Jester, Miller, Weikart). The purpose of presenting attrition analyses at this time is to help the reader evaluate the statistical findings related to educational attainment and the figures shown on nonstandard school progression (special education placement and grade retention). As further statistical analyses are completed updated attrition analyses will accompany them.

In keeping with the precedents established in previous Consortium analyses of longitudinal data (Consortium, 1978; in press), an extensive investigation was made into the phenomenon of attrition in the 1980 follow-up sample used for analyses reported in this report. The methodology employed (for detailed description of attrition methodology, see Appendix A, in 1978 Consortium report or Appendix B, Consortium SRCD Monograph) addresses five questions:

--What percent of the original sample was recovered in the follow-up?

--Were there different rates of attrition for program and control groups?

--Did the final samples differ from dropouts on some important background variable?

--Are different kinds of subjects being selectively retrieved in program versus control groups?

--Did final program samples differ from final control samples on some important background variable?

These questions, addressing the issues, respectively, of rate of attrition, differential rate of attrition, main attrition effect, differential attrition effect, and equivalence of final groups must be answered if inferences are to be made from the analysis of sample statistics.

Rate of Attrition

Table A-1 provides a summation of participants in the 1980 follow-up sample. A participant was defined as a subject for whom any instrument was collected, either School Data Form, Part A or the Youth Interview. The percentage of the original sample participating in the 1980 follow-up is given, by project, in the column labelled "Total Sample". Also given is the number of participants, by project, in the follow-up data collection. The percentages ranged from 63.5% to 93.5%. The remaining projects all sent data for more than 70% of their original samples.

Differential Rate of Attrition

Not only is a high rate of overall recovery necessary for the generalization of results to the original samples, but similar rates of recovery between program and control groups is needed as well. The columns labeled "Control" and "Program" on Table A-1 provide the percent and number of participants in the 1980 follow-up for each project's program and control groups. A Chi-Square tests each project's program and control differential rate of attrition. No p was significant at the .05 level. We can conclude from these statistics that there was not a differential rate of attrition between program and control groups in the 1980 follow-up data.

Main Attrition Effect

To determine if the final samples differed from drop-outs on any important background variables, two-by-two ANOVAs were performed for each project with participation/attrition as one dichotomous factor and program/control status as the other. Four variables were analyzed for main attrition effect: Mother's Educational Level (at the time of the early education project's initiation), Household Hollingshead Socio-Economic Status (also at that time), Pre-Program IQ and Post-Program (6 years old) IQ.

The results of these ANOVAs are given on Tables A-2 through A-5. Because of missing data for certain of these measures for the six projects which were to be analyzed on the four background variables, only 18 of 24 ANOVAs were performed. The p value for each of these separate ANOVAs is given on Tables A-2 through A-5 under the column labelled "Attrition Main Effect." One would expect 2 of these 18 tests, by chance to provide a p less than or equal to .10, and 2 were, Palmer's drop-outs having lower Family Hollingshead SES ratings than the participants (p=.004) and Beller's Post-Program IQ scores being higher for participants than for drop-outs (p=.022). Although both these p-values (.004 and .022) would be significant if taken in isolation, neither is significant if corrected for the fact that they were selected post hoc from 18 values; the corrected p's are .07 and .40.

Selective (Interactive) Attrition

Because of missing data for the background variables analyzed in Tables A-2 through A-5 for several projects, only 13 tests using ANOVA could be made to check for significant interactions between program/control status and drop-out/participant status on the 4 background variables. The p's are given under the columns labeled "p Values Interaction." Of these thirteen tests, one produced an F-score with a probability less than or equal to .05: Levenstein's household SES p=.035 reflecting the original differences. Of the thirteen tests, one would be expected by chance to provide a p less than or equal to .08, and two were, Beller's Pre-Program IQ scores, at .063 and Levenstein's household SES. Neither was significant after correction for multiple tests. From these findings we can conclude that appreciable selective recovery did not take place. The interpretation of this conclusion is that the program groups and the control groups recovered subjects whose background characteristics were comparable to the original program and control samples.

Equivalence of Final Groups

Within the limitations imposed by substantial amounts of missing data on certain background variables, we have answered in the negative the main questions dealing with attrition per se:

There were not different rates of attrition for program and control groups when attrition is defined as failure to be measured on either the 1980 School Data Form or the 1980 Youth Interview.

The final samples did not differ from the drop-outs on important background variables. Different kinds of subjects were not selectively retrieved in program versus control groups.

These findings on attrition allow the generalization of other results of analyses to the original samples. However, the possibility exists that the final program and control groups used for these follow-up analyses were different in respect to some important background variable. This question of equivalence of final groups used in the substantive analyses is not one of attrition per se: observed differences in final groups may be reflecting differences in original program and control samples, and not be due to differences in drop-out rates or drop-out characteristics.

T-tests between final program and control groups provide insight into the question of equivalence of final groups. On Tables A-2 through A-4, the column labelled "Follow-up P/C Comparison" lists probabilities that final groups are different on the background characteristics of Mother's Educational Level, Family Hollingshead SES and Pre-program IQ. (Post-Program IQ is excluded from analysis since a differential in IQ scores between program and control groups after the termination of the early education experience for the program groups was an expected program goal, and was a consistently significant result, both within individual projects and in the pooled p.)

Of the 16 tests for differences between final groups of program and control subjects in the follow-up, one would be expected to be significant by chance at the .063 level of significance, and only one was, Palmer's Pre-program IQ result. This one difference has been considered in detail elsewhere (Consortium, 1978), where we concluded that it may not reflect a real difference between groups, but rather a failure in standardization of the Stanford-Binet IQ test, since the program and control groups were tested at different ages. In any event, this IQ differential was controlled for in the substantive analyses.

Conclusions

Extensive attrition analysis were carried out on the follow-up measures, using a methodology described in detail in Consortium (1978) and Consortium (in press). Presented in this attachment were findings for overall attrition and main and differential attrition effects for 4 background variables, Mother's Education, Family SES, Pre-Program IQ and Post-Program IQ. Findings for main and differential attrition effects for other background variables, such as number of siblings, father presence, were not reported due to both their lack of theoretical importance and the lack of observable attrition effects. The largely negative findings in the attrition analysis were consistent with the hypothesis that attrition was essentially non-systematic, introducing no noticeable biases into the substantive analyses Tests for the equivalence of final program and control groups on background variables were also reported. These tests revealed that, with the one exception discussed and statistically controlled for in the substantive analyses, the final groups were essentially equivalent on the background variables and so could be validly compared on the measures collected on School Data Form A and the Youth Interview.

Table A-1

1980 Follow-up Sample as a Percent of Original Sample by Project
(As of Sept. 30, 1980)

Project	Control % (n)	Program % (n)	Total Sample % (n)	Chi-Square	*p*
Beller	70.3 (78)	74.6 (44)	71.8 (122)	.35	.553
Deutsch[a]	-	-	-	-	-
Gordon/Jester[b]	-	-	-	-	-
Gray	93.8 (45)	93.2 (41)	93.5 (86)	.01	.912
Karnes	75.9 (44)	71.5 (163)	72.4 (207)	.44	.506
Levenstein	86.7 (26)	87.0 (20)	86.8 (46)	.01	.975
Miller	68.4 (39)	62.1 (133)	63.5 (172)	.76	.382
Palmer	76.1 (51)	69.4 (168)	70.9 (219)	1.14	.286
Weikart[b]	-		-	-	-

[a] Deutsch was scheduled to complete selected case studies.

[b] Data collection not complete.

Note. Groups included in the analyses for this table are: Beller -- groups 1, 2, 3; Gray -- groups 1 through 4; Karnes -- Five approaches and controls; Levenstein -- T68 I & II (group 5) and after-only controls (group 19); Miller and Palmer -- all original groups.

Table A-2

1980 General Attrition - p Values and Mean Mother's Educational Level

	p Values			Mean Grades Completed (n)			
				Control		Program	
Project	Attrition Main Effect	Inter-action	Follow-up P/C Compar.	Dropout	Follow-up	Dropout	Program
Beller	.468	---	.782	---(0)	10.69(26)	10.00(3)	10.82(39)
Gordon [a]	---	---	---	---	---	---	---
Gray	.944	---	.541	---(0)	9.14(21)	9.00(2)	8.72(40)
Karnes	.592	.752	.183	11.33(3)	10.74(23)	10.29(17)	10.09([illegible]4)
Levens.	.269	.141	.662	7.74(4)	10.12(26)	11.00(3)	10.40(20)
Miller	.997	.511	.205	10.58(12)	10.19(21)	10.79(68)	10.87(109)
Palmer	.174	.388	.357	11.25(16)	11.33(51)	11.55(66)	11.08(166)
Weikart [a]	---	---	---	---	---	---	---

Note:

Groups included in the educational attainment and school progression analyses were as follows: Beller - groups 1 and 2; Gray - groups 1, 2, 3; Karnes 5 approaches and controls tested for Waves I & II and in school progression analyses the groups included were: Levenstein - T68 I & II (group 5) and after-only controls (group 19); Miller - groups 7 and 8 were excluded; Palmer - all groups included.

[a] Data collection was incomplete at this time for Gordon/Jester and Weikart.

Table A-3

1980 General Attrition - *p* Values and Mean Hollingshead SES

	p Values			Mean Hollingshead SES (n)			
				Control		Program	
Project	Attrition Main Effect	Inter-action	Follow-up P/C Compar.	Dropout	Follow-up	Dropout	Follow-up
Beller	---	---	.969	---(0)	58.10(20)	---(0)	57.95(20)
Gordon	---	---	---	---	---	---	---
Gray	.530	---	.795	---(0)	69.95(21)	73.00(2)	69.52(40)
Karnes	.500	.969	.094	58.33(3)	60.39(23)	63.41(17)	65.22(93)
Levens.	.447	.035	.107	71.00(4)	68.00(26)	55.67(3)	64.60(20)
Miller	---	---	---	---	---	61.40(62)	63.60(103)
Palmer	.004	.453	.291	55.31(16)	57.78(51)	54.53(64)	60.08(165)
Weikart	---	---	---	---	---	---	---

Note: See footnote, Table A-2

Table A-4

1980 General Attrition - p Values and Mean Pretest Stanford-Binet IQ Scores

Project	p Values: Attrition Main Effect	p Values: Interaction	Follow-up P/C Compar.	Mean IQ Score (n): Control Dropout	Mean IQ Score (n): Control Follow-up	Mean IQ Score (n): Program Dropout	Mean IQ Score (n): Program Program
Beller	.259	.063	.505	85.56(16)	93.62(37)	93.86(14)	91.60(43)
Gordon	---	---	---	---	---	---	---
Gray	.527	---	.284	---(0)	85.76(21)	83.33(3)	89.88(41)
Karnes	.429	.509	.155	85.40(5)	90.67(27)	93.32(19)	94.49(97)
Levens.	---	---		---	---	87.33(3)	87.80(20)
Miller	.703	.799	.321	88.15(13)	89.86(21)	91.99(76)	92.48(115)
Palmer	.925	.368	<.001	86.79(14)	84.90(49)	92.83(36)	95.10(84)
Weikart	---	---	---	---	---	---	---

Note: See footnote, Table A-2.

Table A-5

1980 General Attrition - p Values and Mean Postprogram IQ Scores (Age 6)

	p Values		Mean IQ Score (n)			
			Control			
Project	Attrition Main Effect	Inter-action	Dropout	Follow-up	Dropout	Program
Beller	.022	.572	85.56(16)	93.62(37)	95.07(14)	99.76(42)
Gordon	---	---	---	---	---	---
Gray	.788	---	---(0)	82.78(18)	88.00(1)	95.15(41)
Karnes	---	---	---	---	107.43(14)	104.44(90)
Levens.	---	---	---	---	---	---
Miller	.344	.530	90.90(10)	95.10(21)	93.19(62)	94.37(115)
Palmer	---	---	---	---	---	---
Weikart	---	---	---	86.34(64)	---	91.25(56)

Note: See footnote, Table A-2.

Response Rates and Final Dispositions

In order to calculate response rates for the follow-up sample, the final disposition of each ID case number in the three sites (Beller, Gray, and Karnes) was assigned to one of the following categories.

(C) Completed = Code 1

(R) Refused to give permission, refused to be interviewed = Code 6

(U) Located but unable to test because:

Moved = Code 4

Unable to test, keep appointment, death of respondent, etc. = Code 5

Terminated, unable to complete test = Code 7

(L) Lost, unable to trace on records = Code 3

(A) Attempted to locate and test: (U) + (L) = Codes 3 + 4 + 5 + 7

(OS) Out of sample

Wave not scheduled to test at this follow-up = Code 2

Dropped from sample at previous follow-up = Code 8

No data, unknown disposition = Code 9

Response rates are defined as the number of final dispositions in a given category divided by the number of cases in the total sample or subsample. The three response rates computed for completion and refusal categories are shown below:

Response Rate_1: Disposition divided by total possible cases

$$\text{e.g., } CR_1 = \frac{C}{C + R + A + OS}$$

Response Rate_2: Disposition divided by attempts (excludes out of (samples)

$$\text{e.g., } CR_2 = \frac{C}{C + R + A}$$

Response Rate$_3$: Disposition divided by actual cases contacted (excludes lost)

$$\text{e.g., } CR_3 = \frac{C}{C + R + U}$$

The Combined Response Rate Computations and Final Dispositions by Instrument for the three project sites are shown in Table A-6. In both Tables A-6 and A-7 response and refusal rates are calculated only for those groups of respondents being analyzed in this follow-up; that is, Beller groups 1 and 2, Gray groups 1, 2 and 3, and Karnes groups 1, 2, 3, 6, 7, 10 and Wave I and II of the control groups.

As can be seen in Table A-6 the response rates for School Data Forms A are higher than for the Life Status Interview. This is consistent with the response rate pattern on our previous follow-up. It is a known research phenomenon to have lower response rates on data collections involving direct participation of respondents, than on data collected from records and files. In the original data file there were 333 subjects in the groups noted above. There were 277 complete school data forms out of 333 possible, resulting in a completion rate (1) of 68.2%. The completion rate (2) of 88.5% is based on the number of records attempted to locate. The completion rate (3) of 95.8% is based on the number of records that could be secured once located. The refusal rate (1) of .6% is the number of refusals to access School Data Forms from this follow-up sample (N = 333). The refusal rate (2) also .6% eliminates from the denominator subjects out of sample and for whom data is missing. The refusal rate (3) of .7% is the rate of refusal on the school records actually located.

The completion rate for the Interview follows the same procedure. Completion rate (1) is 69.1% and represents 230 completed interviews out of the total possible on this follow-up, 333. Completion rate (2) of 73.5% is based on the number of subjects attempted to locate and interview and completion rate (3) of 89.1% is the percentage number of completions out of those subjects actually located. As mentioned previously, the refusal rate on the Life Status Interview is predictably higher than the comparable rate for School Data Form A. There were 13 refusals out of the 333 original subjects for a refusal rate (1) of 3.9%. The refusal rate for those subjects attempted to locate is 4.1% and for subjects actually located the refusal rate (3) is 4.5%. These refusal rates for minority subjects ages 18 - 22 are acceptably low.

Table A-7 is the breakdown by site of the completion and refusal rates. The same six equations as were used previously were repeated. The completion rates (3) for the three sites on the School Data Form A are all in the 90% - 100% range, and the completion rate (3) on Life Status Interviews range from 86.3% - 100%. All sites have low refusal rates on School Data Form A, but there is more variation on the refusal rate for Interviews. Researchers at the Karnes site formed a post hoc control group for this follow-up. Because those subjects were not involved in the research venture during the past they may have a smaller stake in the process and therefore reduced motivation to be interviewed. This reduced motivation is offered as an explanation of the difference in refusal rate at the Karnes site on the Life Status Interview. In summary, the refusal percentages are acceptably low, especially given the variation in group formation at the three sites.

Table A-6

Beller, Gray & Karnes Combined*	School Data Form, Part A N	%	Interview N	%
(1) Completion $CR_1 = \frac{C}{C + R + A + OS}$	277	68.2	230	69.1
(2) Completion $CR_2 = \frac{C}{C + R + A}$	277	88.5	230	78.5
(3) Completion $CR_3 = \frac{C}{C + R + A}$	277	95.8	230	89.1
(1) Refusal $RR_1 = \frac{R}{C + R + A + OS}$	2	.6	13	3.9
(2) Refusal $RR_2 = \frac{R}{C + R + A}$	2	.6	13	4.1
(3) Refusal $RR_3 = \frac{R}{C + R + A}$	2	.7	13	4.5

*Rates only for groups in 1980 analysis:
Beller - Groups 1 & 2
Gray - Groups 1, 2 & 3
Karnes - Groups 1, 2, 3, 6, 7, 10 & Control Groups Waves I & II.

Table A-7

Response Rate Computations and Final Dispositions by Project

Beller (Groups 1 & 2 combined)*	School Data Form, Part A		Interview	
	N	%	N	%
Beller (Groups 1 & 2 combined)*				
(1) Completions $CR_1 = \frac{C}{C + R + A + OS}$	81	72.3	63	56.2
(2) Completions $CR_2 = \frac{C}{C + R + A}$	81	88	63	85.1
(3) Completions $CR_3 = \frac{C}{C + R + U}$	81	90	63	87.5
(1) Refusal $RR_1 = \frac{R}{C + R + A + OS}$	2	1.8	2	1.8
(2) Refusal $RR_2 = \frac{R}{C + R + A}$	2	2.2	2	2.7
(3) Refusal $RR_3 = \frac{R}{C + R + U}$	2	2.2	2	2.7
Gray (Groups 1,2 & 3 combined)*				
(1) Completions $CR_1 = \frac{C}{C + A + R + OS}$	62	95.4	60	92.3
(2) Completions $CR_2 = \frac{C}{C + R + A}$	62	98.4	60	98.4
(3) Completions $CR_3 = \frac{C}{C + R + U}$	62	100.	60	100.
(1) Refusal $RR_1 = \frac{R}{C + R + A + OS}$	0	0	0	0
(2) Refusal $RR_2 = \frac{R}{C + R + A}$	0	0	0	0
(3) Refusal $RR_3 = \frac{R}{C + R + U}$	0	0	0	0

Table A-7 (Cont.)

Response Rate Computations and Final Dispositions by Project

		School Data Form, Part A		Interview	
		N	%	N	%
Karnes (Groups 1, 2, 3, 6, 7, 10 and wave I & II Controls)*					
(1) Completions	$CR_1 = \frac{C}{C + R + A + OS}$	134	85.9	107	68.6
(2) Completions	$CR_2 = \frac{C}{C + R + A}$	134	85.9	107	68.6
(3) Completions	$CR_3 = \frac{C}{C + R + U}$	134	99.2	107	86.3
(1) Refusal	$RR_1 = \frac{R}{C + R + A + OS}$	0	0	11	7.
(2) Refusal	$RR_2 = \frac{R}{C + R + A}$	0	0	11	7.
(3) Refusal	$RR_3 = \frac{R}{C + R + U}$	0	0	11	8.9

* these are groups in the 1980 analysis.

ATTACHMENT B

THE KARNES PROJECT

A new subsample has been added to the Consortium data base. The absence of a control group in the Karnes Project has severely limited the utility of their data in the Consortium follow-up analyses. In 1979, it was decided to investigate the feasibility of developing an ex post facto control group. The arguments for and against development of a post hoc control group are presented in this section. A control group has been developed thus making Karnes' data more fully utilizable in the 1980 analyses. Following, is a description of the Karnes Project, the sample selection procedures for the children whose data are used in 1980 analyses, and the procedures followed in developing the ex post facto control group. Comparisons of the treatment and control groups on baseline measures are presented, as well as comparisons of the two waves of subjects. We have described only the Karnes Project in this report; descriptions of the sample selection procedures for the Gray and Beller Projects can be found in previous reports (see Consortium Annual Report, 1977 and 1978; also SRCD Monograph, Attachment E).

As part of a larger project designed to develop and test curricula for young children from low-income families, Dr. Karnes performed a study to evaluate the differential effects of five preschool programs. The five programs selected for comparison were Bereiter-Englemann, Karnes Ameliorative, Montessori, Community-Integrated, and Traditional nursery school models. The five programs differed in the degree of structure, as judged by the nature of teacher-child interaction; the Traditional and Community-Integrated programs were least structured; the Montessori program incorporated considerable structure; the Karnes Ameliorative and the Bereiter-Engelmann programs were highly structured. The major goals of the Traditional nursery school (Group 1) were to promote the personal, social, motor and general language development of the children. Teachers were to capitalize on opportunities for incidental and informal learning; music, story and art activities were scheduled regularly. The Community-Integrated program (Group 7) provided a traditional nursery school experience for the children. The research children attended on-going programs in the community that were composed predominantly of middle- and upper-class white children. The Montessori program (Group 6) met Montessori standards for staff and classroom materials. Children were allowed some "spontaneous choice" of approved materials as well as periods of music, games, "practical life" demonstrations and field trips. In the Ameliorative program (Group 3), manipulative and multi-sensory materials provided the framework for eliciting the verbal responses necessary for language development (which was considered to be a critical area of deficit for disadvantaged children). Content to be learned was presented in a game format. In the Direct Verbal (Bereiter-Engelmann) program (Groups 2 and 10) intensive oral drill in verbal and logical patterns was the mode for instruction; a deliberate effort was made to minimize the use of visual and manipulative materials. Each day there were three 20-minute small group instructional sessions focusing on language, arithmetic, and reading.

Sample Selection

The children in this study (whose data were included in FY1980 analyses) were drawn in two waves from economically depressed neighborhoods of Champaign-Urbana, Illinois, a community of 100,000. Families, judged by public aid and school authorities to be economically and educationally deprived, were approached to determine whether they had children who would be 4 years of age before December 1 of that year and who had no previous preschool experience. In addition, interviewers canvassed certain acutely disadvantaged sections of the city to locate children unknown to the referring agencies. Final eligibility for the program was determined on the basis of detailed family histories collected by home interviewers. The 1960 Stanford-Binet Intelligence Scale was administered to eligible children who were then stratified, on the basis of their intelligence quotients, into three groups of IQ scores: 100 and above, 90-99, 70-89. The children were assigned to class units (*N*=15) in which one-third of each class consisted of children who had scored in the "high" IQ range; one-third in the "middle" range; and one-third in the "low" range. Mean intelligence quotients were then computed for the three strata and for each class unit. These means were evaluated for comparability between class units as a whole and for strata between classes. These strata insured a balanced range of intelligence scores in each class unit and provided an opportunity to evaluate the effectiveness of the various programs on children from different ability groups. Class units were examined to assure comparability of sex and race. When necessary, substitutions were made between classes to maintain an approximate ratio of 67% black children and 33% white children and a ratio of approximately 50% male and 50% female children. Finally, each class unit was randomly assigned to a particular intervention program. The initial composition of the groups is summarized in Table B1.

Class units of 15 children were developed in two waves. In the first wave, 1965-66, two class units were assigned to the Traditional program (Group 1), two to the Ameliorative program (Group 3), and one class unit to the Bereiter-Engelmann or Direct Verbal program (Group 2). The three class-units in the second wave, 1966-67, were assigned, one each, to the Community-Integrated program (Group 7), the Montessori program (Group 6), and to the Bereiter-Englemann program (Group 10). The children attended one of the preschool models 2 hours a day for 7-8 months. No comtemporaneous control groups for these programs were developed by Karnes.

Development of the Control Group

The generation of an ex post facto control group is, at best, a risky proposition. It is an experimentally unsound procedure because of the inability to assure random assignment of subjects to experimental and control conditions. It is never possible to be absolutely certain that some differences did not exist between experimental and control children. The most positive element in defense of the development of an ex post facto control group is the procedure followed in forming the experimental groups during the time period of the initial research. As was discussed earlier, Karnes' treatment group children were selected so as to meet the following conditions: approximately half of the children in each group were male and half were female; approximately two-thirds of the children in each group were black and one-third were white; and each group consisted of three stratified Stanford-Binet IQ subgroups of approximately equal size. A number of these stratified "matched" groups were formed each year of the project. To accomplish this, it was necessary to test a larger number of children every year than for whom programming could be provided. Generally, 30% more children were tested than would be assigned to treatment groups. Moreover, after children were assigned, it was often necessary to test additional children (a) to fill out all of the groups with proper stratification and (b) to replace children who were unable to attend or who dropped out early in the year. This usually involved additional testing to locate a child who would fill a specific characteristic slot. All of this resulted in the testing each year of a number of children who did not receive programming; this subject pool is ideal for the formation of an ex post facto control group.

In 1980, the effort to develop a control group commenced. A review was made of the records of children who were screened and tested for admission to the Karnes Project. A list was made of those children who satisfied the experimental sample contitions but who were not assigned to a preschool program[1]. This list of potential controls contained between 200 and 400 names. The folders of these children were examined for those who satisfied the following criteria: they were tested during the appropriate time period (1965-67); they were tested on at least the Stanford-Binet Intelligence Scale; there was no indication that the child was ineligible for the program; the child did not attend any preschool or day care program. Forty children satisfied these criteria[2]. Of these 40 children, 8 were subsequently excluded from the control group (Group 0) because they did attend one of the five-approach programs for a short period of time before withdrawing[1]. Because these eight children were briefly exposed to the benefits of a preschool program and because the greatest benefits tend to appear early, it was decided they should not be included in the optimal control group. The ex post facto control group, then, consists of 32 children. Eighteen of these children were tested for the 1965-66 school year, and thus belong to Wave 1; the other 14 were tested for the 1966-67 school year and belong to Wave 2[2].

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[1] The primary reason for nonassignment to a program at that time was that there were too many children of that particular sex-race-IQ strata. In a few instances, children were excluded because they lived outside the bus transportation area or the parents refused services.

[2] Most children were excluded because no test data were in the folders. A smaller number were excluded because the children attended some other preschool program.

Comparison of Groups on Baseline Measures

The groups being used in 1980 analyses are the program children in the Five Approaches study and a subset of the total ex post facto control group. Program children were assigned to the five programs in 2 waves. In 1965-66, Groups 1, 2 and 3 (Traditional, Bereiter-Englemann, and Ameliorative) attended preschool; these groups constitute Wave 1. In 1966-67, Groups 6,7, and 10 (Montessori, Community-Integrated and a second Bereiter-Englemann) attended programs; these 3 groups are Wave 2 of the program set. For purposes of baseline comparisons, the control group (Group 0) was also divided into 2 waves on the basis of year of eligibility for (and nonassignment to) an early education program. The following comparisons (see Table B-1) are performed on waves 1 and 2 of the optimal control group

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[1] Five of these children withdrew because the family moved or the mother became ill; three withdrew because the mother refused to allow the child to continue.

[2] Data were received from the Karnes project for a total of 58 control children. Forty of these have been accounted for in the text above. An additional 14 children meet all the requirements of the optimal control group, excepting the year of testing; 12 children belong to Wave 3 of the Karnes Project (1967-68) and 2 belong to Wave 4 (1968-69). (It has recently been brought to our attention that Wave 3 children, although recruited in 1967-68, are the same age as Wave 2 children. The analyses contained in this report do not include Wave 3). The 4 children not yet mentioned, belong to Waves 3 & 4 and they attended one of the programs for a short period of time, then withdrew; for these reasons, they were excluded from the present analyses.

(Group 0) and those program children in The Five Approaches Study for whom there is any 1980 follow-up data; these are the subjects included in FY1980 Karnes' project analyses.

Statistical tests were performed to answer two different questions: Do the two waves of subjects differ on any of the baseline measures? and Do program and control subjects differ on the baseline measures? The baseline measures are preprogram IQ (Stanford-Binet), mother's education (in years), household SES (Hollingshead), number of siblings, gender (percent female), father present vs. absent (percent present), and race (percent black).

There is potential for the waves to differ, in that they were recruited at two different points in time, a year apart. Moreover, the two waves in this 1980 control group are subsamples of larger groups of nonassigned children from 1965-66 and 1966-67. These subsamples were not selected randomly, but rather on the basis of test scores being present in their folders. Here again it is possible that the children with test scores available in Wave 1 are different from those in Wave 2. Similarly, the 2 waves of the program group could have been subject to selective attrition.

Summary statistics of the seven baseline measures are presented in Table B-1, columns 1-4. Two-factor ANOVAs tested for main effects of wave (2-tailed probabilities are shown in column 5), main effects of program (see column 6) and any interactions between wave and program (column 7). In column 7 it can be seen that the interactions between waves and the program/control factor are not significant on any of the baseline measures (the probabilities range from .070 to .907). Because of this, any main effects are interpretable in a straightforward manner. The effects of wave (column 5) are not significant on any of the baseline measures (p's range from .075 to .432). Despite the fact that the subjects were recruited in two different years and the two waves in the final control group and program group could have been differentially retrieved, these statistics indicate the waves are not different. Since the waves do not differ, it is appropriate to combine the two waves on each of the baseline measures, before testing for program/control differences. Because it is not necessary to treat waves 1 and 2 as separate groups, the ANOVAs on main effect of program (column 6) are not the best test of program/control differences on baseline measures and will not be discussed. The more appropriate test of differences is a simple t-test; the results of these are reported in column 9. The t-tests indicate that the program and control groups do not differ on pretest IQ ($\underline{p}$ = .183), or on household SES ($\underline{p}$ = .094). The groups do not differ in their gender distribution ($\underline{p}$ = .111) or in racial distribution ($\underline{p}$ = .669). When the children were four years old, the groups did not differ on the percent with father present in the home ($\underline{p}$=.987). The groups did differ on number of siblings (measured when the children were four years old); children in the control group had significantly more siblings than did children in the program group (p = .027). However, this difference is not significant after correction for multiple comparisons. Overall, we can conclude these two groups are very well-matched and this ex post facto group is an appropriate comparison group for determining the effects of early education on later outcomes.

Table B-1

Karnes Five Approaches and Appropriate Control Groups Comparisons on Baseline Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Program		Control		Wave Main Effect Signif. (2-tail)	P-C Main Effect Signif. (2-tail)	Interaction F-test Signif. (2-tail)	P-C t-test Signif. (2-tail)
	Wave 1	Wave 2	Wave 1	Wave 2				
Stanford-Binet IQ Pretest	95.23 (70)	92.89 (46)	91.83 (18)	87.29 (14)	.119	.055	.627	.155
Mother's Ed. (in years)	9.79 (67)	10.61 (44)	11.07 (15)	10.45 (11)	.075	.084	.070	.183
No. Siblings	3.61 (69)	3.16 (44)	6.00 (8)	4.00 (11)	.199	.009	.187	.027
Gender (percent female)	49% (70)	57% (46)	61% (18)	71% (14)	.291	.177	.907	.111
Father present (percent)	55% (69)	67% (43)	64% (14)	50% (10)	.388	.876	.240	.987
Race (percent black)	61% (70)	67% (46)	56% (18)	64% (14)	.432	.632	.889	.669

Note: P = program group
C = control group

ATTACHMENT C

PROPOSED MODEL AND SYSTEMS OF EQUATIONS for SCHOOL COMPETENCE, EDUCATIONAL ATTAINMENT, AND LABOR MARKET EXPERIENCES OF CONSORTIUM SUBJECTS

Our proposed model conceptualizes achievement of status attainment as occurring within two societal institutions: the formal educational system and the labor market. The role of early education programs will be investigated at each stage of status achievement. The formal model can be summarized as a series of four recursive equations: shown in Table C-1. The independent variables (antecedents) are shown on the right side of the equation. The sources and definitions of the dependent and independent proposed variables are shown in Tables C-2 and C-3.

Table C-1

Equations for School Competence, Educational Attainment and Labor Market Experiences

Early Mental Ability = Preschool + SEX + SES + Household Composition + Residual

School Competence = Preschool + SEX + SES + Household Composition + Early Mental Ability

Educational Attainment = Preschool + SEX + SES + Household Composition + Early Mental Ability + School Competence + Suspensions + Pregnancy during High School + High School Track + Attitudes and Values of Subject + Family Social Psychological Environment

Labor Market Outcomes = Preschool + SEX + SES + Household Composition + Early Mental Ability + School Competence + Educational Attainment + Suspensions + Subject Achievement-Orientation + Parental Expectations + Subject's Children + Previous Work Experience + Interactions of Sex and Selected Independent Variables

Table C-2

Dependent Variable Definitions

A. Early Mental Ability (collected between 1962 and 1967)

1. Data - Stanford-Binet IQ Score collected annually between the ages of 6 and 8 (data source: tests individually administered by research project)

2. Variable specification

    a. IQ at age 6 (entry into first grade)

    b. Average IQ between the ages of 6 and 8

B. School Competence (collected in 1979)

1. Data

    a. Assignment to special education -- ever in school career

    b. Retention in grade -- ever in school career

    c. Course marks

    d. Track

    (Data source: from school records)

2. Variable definition: 1 = never assigned or retained; 0 = assigned to special education or retained in grade

c. Educational Attainment (collected in 1979)

1. Data

    a. High School diploma/no high school diploma

    b. Never dropped out/dropped out once or more

    c. Current or past enrollment in college of other post-secondary education program

    d. Highest grade completed

    (Data source: a & b from school records, verified by 1979 youth interview; c from 1979 youth interview)

Table C-2 (Cont.)

2. Variable definition
    a. High school or GED diploma/did not graduate
    b. Attainment scale where: 1 = no diploma; 2 = high school diploma; 3 = diploma and some post-secondary education (< 1 year); 4 = completed or currently in post-secondary program or was in a program for at least a year

D. Labor Market Outcomes (collected in 1979 Youth Interview)

1. Data
    a. In/out of labor force currently
    b. Employed/Unemployed currently
    c. Main source of income where 1 = self or spouse's work; 2 = other
    d. Hourly wage of current or last job
    e. Monthly earnings
    f. Prestige rating of current or last job (Hollingshead or NORC scale, skilled vs. unskilled); or other categorizations such as sex stereotype of job, job complexity
    g. Length of employment at current or last job (in months)
    h. Job satisfaction (1 = very satisfied; 5 = very dissatisfied)
2. Variable definition
    a. employed/unemployed
    b. employed/unemployed or not in labor force
    c. main source of income where 1 = self or spouse's work; 2 = other
    d. hourly wage of current or last job (Hollingshead or NORC scale, skilled vs. unskilled) 1 or other categorizations such as sex stereotype of job; job complexity

e. length of employment at current or last job (in months)

f. job satisfaction (1 = very satisfied; 5 = very dissatisfied)

Table C-3

Independent Variable Definitions

A. Preschool Participation

  1. Data: whether child participated in a Consortium preschool
  2. Variable definition: 1 = program; 0 = control

B. Sex

  Data and variable definition: 1 = female; 0 = male

C. SES

  1. Data
     a. Hollingshead 2-Factor ISP of household head (collected 1966 in first wave)
     b. Mother's education in grades completed (collected sometime in 1960's in first wave)
     c. Hollingshead occupational code for father and mother (collected in 1976)
     d. Mother's education in grades completed; father's education where available (collected in 1976)
     e. Type of housing -- own vs. rent; living in public housing yes/no (collected in 1976)

     (Data source: intake interviews; 1976 Parent Interview)
  2. Variable definition: our previous research has utilized variables a and b. Later indicators of SES (c through f) are available.

Table C-3 (Cont.)

D. Household Composition

1. Data

   a. Father presence/absence collected in first wave

   b. Number of siblings collected in first wave

   c. Family size (1976 Parent Interview) or household size

   d. Adult/child ratio (1976 Parent Interview)

   e. Living with parents yes/no (1979 Youth Interview)

   f. Married/not married (1979 Youth Interview)

   g. Whether subject has children -- male and female subjects' report on whether they have children and whether their children are living in subject's household or elsewhere (1979 Youth Interview)

   h. Pregnancies during high school (1979 Youth Interview and 1979 school records)

   (Data source: intake interviews 1962-66; 1976 Parent Interview; 1979 Youth Interview and school records)

2. Variable definition: The definition varies by the outcome measures because of the need for temporal precedence of independent variables. For early mental ability and school competence outcomes, a and b can be utilized. For educational attainment, the 1976 household composition specified in the Parent Interview can be utilized, while for labor market outcomes both 1976 and 1979 data on household composition can be utilized. Whether subjects were pregnant during high school is included in the educational attainment model for females. Whether male or female subjects

Table C-3 (Cont.)

have had children will be included in the labor market outcomes model.

E. Suspensions

1. Data

a. Number of suspensions

b. Reasons for up to three suspensions

(Data source: Collected from 1979 Youth Interview, verified by school records)

2. Variable specification

a. Number of suspensions

b. Possible index of reasons for suspensions

F. High School Track

1. Data

a. Track for elementary years

b. Track for high school

(Data source: school records collected in 1979)

2. Variable definition: model high school track with three categories: 1 = vocational/technical; 2 = regular/general; 3 = college prep

G. Family Social-Psychological Environment

1. Data

a. Family mobility -- how many times family moved from when child was 5 years to approximately 18 years old (1976 Parent Interview)

b. Family relationships -- where and whether family engages in activities together, how well subject gets along with family members, if subject listed parent as most admired adult (1976 Youth Interview)

c. Family stability -- ever lived with persons other than parents (1976 Youth Interview)

d. Living conditions -- whether subject has private room; household density (rooms per person) (1976 Parent Interview)

e. Parental educational and occupational expectations for subject; parental satisfaction with subject's school performance (1976 Parent Interview) for Labor Outcomes analysis

(Data source: 1976 Parent and Youth Interviews)

2. Variable definition: For Educational Attainment model -- variables a through d family mobility, family relationships, living conditions, family stability will be tested. For Labor Market Outcomes -- e parental expectations can be tested. Composite indices can be created where appropriate before multivariate analyses are done.

H. Attitudes and Values of Subject

1. Data

a. Attitudes toward school -- what's best/worst things about school (1976 Youth Interview)

b. Self-evaluation -- perception of school performance relative to peers (1976 Youth Interview)

Table C-3 (Cont.)

c. Achievement-orientation -- pride in an accomplishment related to achieving; admires adult for educational or occupational related attributes (1976 Youth Interview)

d. Role model -- does subject specify an adult role model? (1976 Youth Interview)

e. Self report of deviance -- worst trouble subject reports in school and other contexts (1976 Youth Interview)

f. Occupational and educational aspirations and expectations (1976 Youth Interview)

2. Variable definition: For model of Educational Attainment, we can correlate (a) through (e) attitudes and values of subject; for model of Labor Market Outcomes, we can test variables (c) and (f). Composite indices can be created where appropriate before multivariate analyses are done.

I. Previous Work Experience

1. Data

a. Age at first full-time job (collected in 1979)

b. Age at first part-time job (collected in 1979)

c. Number full-time jobs (collected in 1979)

d. Number part-time jobs (collected in 1979)

e. Length of employment at last job (if not employed) or previous job (if employed) (collected in 1979)

f. Prestige ranking of last job (if not employed) or previous job (if employed) (collected in 1979)

Table C-3 (Cont.)

g. Participation in special vocational training programs such as on-the-job training, work/study experiences in high school, military experience, government sponsored programs (e.g., CETA)

(Data source: 1979 school records; 1976 and 1979 Youth Interviews)

2. Variable definition: to be decided after univariate analysis of above variables

Attachment D

Instruments

Attachment E

Publications, Newspaper articles

ATTACHMENT F

ANALYSES OF THE DEUTSCH CASE STUDIES

Included in this attachment are two separate analyses of case studies of program participants in the Deutsch project. The analyses performed by Consortium staff on case studies prepared by the Deutsch staff are presented in Part A. They are being used by the Consortium staff as background material for the task of operationalizing the model and the variables for the forthcoming investigation of educational attainment and labor market outcomes, as described in the body of this report and attachment C. The Deutsch IDS staff prepared the case studies and summary of salient themes presented in Part B of this attachment.

ATTACHMENT F (PART A)

QUALITATIVE ANALYSIS of FACTORS in EDUCATIONAL ATTAINMENT and LABOR MARKET EXPERIENCES: A DISCUSSION of "FINAL OUTCOMES" in the DEUTSCH CASE STUDIES

As the subjects involved in many of the Consortium programs reached young adulthood, interest began to arise among the investigators concerning the possible association of early education with such "final outcome" variables as graduation from high school, college attendence, successful employment, type of employment and the achievement of a self-supporting life style. Earlier analyses (Consortium, 1977) had revealed the existence of a positive early education effect well beyond the termination of treatment in the lower retention in grade rate and placement in special education rate of program participants in comparison to their controls. These findings lent plausibility to the hypothesis that an early education effect might be found in variables whose measurement was even more removed in time from termination of treatment, such as educational attainment and labor market success.

Towards the end of testing this hypothesis, the Consortium staff has conceptualized a model and system of educations for the association of early education and certain background variables with "final outcomes." The model consists of four recursive equations to be solved simultaneously, with early mental ability, school competence, educational attainment and labor market outcomes functioning as the dependent variables. (For a complete description of the model, see

Initial investigations have been made into the association of early education and background variables with educational attainment. A discussion of preliminary findings, based on analysis of high school dropout rates of treatment and control subjects in the three programs whose subjects are old enough to be age-eligible for graduation from high school, can be found in Consortium Annual Report, 1980 . However, many important variables in the educational attainment equation, such as the subject's attitudes and values and the family's social-psychological environment, lack strict operationalization at present. Similarly, variables in the labor market outcomes equation such as parental expectations, previous work experiences and even the dependent variable labor market outcomes, have not been explored well enough to produce a functional rating scheme.

In an attempt to facilitate operationalization of these two equations for quantitative investigations (the early mental ability and school competence equations having been operationalized and investigated in Consortium, 1977), a qualitative analysis was carried out on six case studies prepared by the Deutsch staff. The purpose of the analysis was to provide Consortium staff with a probe of some of the factors of the general educational and vocational environment in which young adults from lower economic classes must function. It was not the intent of this analysis to suggest specific measures and rating schemes for dependent or independent variables, but rather to highlight certain factors in educational attainment and labor market outcomes which may be important to consider in operationalizatio

The six case studies were drawn from interview data gathered over a period of 18 years by the Deutsch staff. Four interviews were available for each subject:

(1) interview with subject's mother in her home at the time of program entry (1962-1964);

(2) interview with subject's mother in her home, 13 to 15 years later, when subject was age-eligible to be in high school (1977);

(3) interview with the subject during the same year (1977);

(4) interview with the subject at the Deutsch staff office two years later (1979)

The subjects for the six case studies were selected non-randomly from the Deutsch treatment group. An attempt was made to select both males and females, both apparent successes (in terms of the final outcome variables) and failures. Subjects were also selected on the basis of having very little missing data from the four standardized interviews, in order to provide comparability between them on salient variables.

Following review of the case studies in their narrative form, 21 variables were chosen for comparison and tabulation. Table F-1 contains the information available on those variables for the six fictitiously named subjects.

SEX, AGE: The subjects included 4 males and 2 females, all of whom were old enough by the time of the final interview to have completed high school.

HS GRADUATE: Only Allan graduated from high school, and he had attended private religious junior and senior high schools as a direct result of encouragement and intervention by a Deutsch enrichment project guidance counselor. Five of the subjects had experienced serious difficulty in junior or senior high, being suspended or expelled for truancy, fighting, disrputive conduct or retained in grade.

ATTENDS COLLEGE: Allan had attended two years of a business college.

ENROLLED NOW: Besides Allan, who had just returned to college from a year's leave of absence, only Carl was enrolled in an educational program, a high school equivalency course taught at a community college.

PLANS TO RETURN: In addition, Brenda and Frank had taken concrete steps toward taking the high school equivalency test. They had signed up for the test, but were not formally studying in preparation for it.

WANTS TO RETURN: The remaining subjects expressed a desire to either enroll in an equivalency program or take the equivalency test, but had taken no concrete steps towards that end.

WORKING NOW: Earl and Frank had full-time, low-paying jobs (at or below the legal minimum wage) with little prospect for advancement. Allan supported his college expenses by scalping tickets at music and sports events. The remaining 3 subjects were not working at all, Brenda because she was caring for her young daughter and Carl because he was a full-time student.

WORKED IN PAST: All the males had worked during their high school years. Both of the men enrolled in educational programs had worked full-time as clothing salesmen during absences from their program. The two women had no work experience.

PARENTS' BIRTHPLACE: The four subjects with data available on this variable had parents who identified themselves as "Southern" in upbringing.

PARENTS EDUCATION: Allan's mother had attended high school equivalency classes while he was in school, and Brenda's mother had graduated from high school before moving to New York City. Only Allan's mother had attended college. She was enrolled at the time of the second parent interview.

PARENTS' INCOME: While five of the subjects' families had relied heavily on publicly-provided social services for support at some point during the child's life, only one family had never received the majority of its income at some time from employment. Types of employment cited were nurse's aid, watchman, waiter, administrative assistant at a social service agency and domestic.

SOURCE OF SUPPORT: Earl and Frank, with full-time jobs, were self-supporting. Brenda's husband in the air force provided with her a source of support independent of her mother, although she also relied on social services. The other three subjects were still financially dependent on their families (two of which were themselves primarily dependent on social services).

HAS OWN CHILDREN: Three of the subjects had children, although only Brenda had her child living with her and was married.

SUBJECTS' RESIDENCE: All of the subjects were living at the homes of their parents or other close relatives.

SIBLING OUTCOME: Data was collected on the "final outcomes" of siblings who were old enough to be age-eligible for high school graduation. Three of Allan's four eligible siblings, as well as his mother and himself, were enrolled in college, having received high school degrees. Brenda's one and Carl's two eligible siblings had high school degrees and were enrolled in college or college-level training programs. Both of Denise's eligible siblings, like her, were high school drop-outs.

PARENTS' EXPECTATIONS: At the initial interview, mothers were asked what they expected their children to achieve in school or the work world. The question was repeated at the second parent interview during the child's high school aged years. During the initial interview, all the mothers responded with jobs requiring extended college education. When questioned about the realistic nature of their expectations, they uniformly expressed belief that their child would at least graduate from high school. At the second interview, all of the boys were still in high school, and their mothers continued to expect that they would attend college upon graduation. Vocations cited were engineer, electrician, architect and preacher. Both girls had dropped out by the time of the second interview, but their mothers expected them to return, graduate, and work as nurses.

CHILD'S EXPECTATIONS: The subjects themselves were questioned on their expectations during their high school aged period and at the post-high school age interview two years later. All except Denise had expectations which included finishing high school, either through equivalency programs and tests or through enlistment in the armed forces. Only Allan, who already was in college, and Earl were clearly interested in enrolling in college, while Brenda and Carl had specific post-high school vocational training programs in mind.

From this review of subject and family characteristics, some points which need to be considered more carefully emerge.

First, in regards to the operationalization of the educational attainment variable, these case studies suggest that public high school is a very discouraging and difficult experience for students in urban, low-income areas. However, it should be noted that the subjects displayed tenacity in trying for their diplomas. All five drop-outs re-entered some form of educational program. Brenda left school shortly before her daughter's birth when she was in tenth gaade, and attempted twice to return to school, discovering that the combination of mother and student was too taxing for her. Carl dropped out after repeating tenth grade, complaining that school was 'boring and that the teachers 'just tried to push you along' regardless of difficulties. He enrolled in his equivalency program at a community college soon after, though, and found himself much more comfortable in a situation where he could 'pace himself.' Denise, too, found school 'boring' and dropped out in the eighth grade. See also enrolled, albeit briefly, in an equivalency program. Earl was expelled from high school for fighting with a security guard and enrolled in an equivalency program before going to work full-time. Frank complained that in high school he was 'bored' and that the 'teachers didn't care', and even failed to contact his parents when he was truant for long periods of time. He studied for and took the high school equivalency test after dropping out, but failed in his first attempt.

Allan, who attended private junior and senior high school, is the only one of the subjects with a high school diploma or college attendance. He reported liking the "strict rules and values" of the religious schools, and felt that "as for his attending college, his family pushed and it was expected" of him. When he had difficulty in his sophomore year of college, he relied on his family's encouragement during his year's leave of absence to rebuild his confidence to return.

Although only one of the subjects had graduated from high school by 1979, graduation and attendance at a post-high school educational institution is a possibility well within the reach and in the consciousness of almost all of them: many of their siblings graduated from high school and went on to college or training programs. Only Denise, whose difficulties started in school so early, doesn't see herself with a high school diploma someday.

In regards to labor market experiences, it is interesting to note that all the male subjects had part-time work experience during their high school years, whereas the females did not. At the time of the 1979 interview, all of the male subjects were either working or in an educational program. The possibility of advancement, or even for a pay rate at the legal minimum wage, was not great for the two employed men. Their estimation of the vocational environment, as revealed in their responses to interview questions and in their own plans for the future, appears to be that a high school diploma would not be of much use to them in terms of job type and the possibility for advancement. However, there is a feeling on the part of many of the subjects that vocational programs, which themselves require a high school degree for entry in many cases, do provide access to jobs in health care, clerical professions and building trades which can expand into careers.

Finally, the review of the case studies suggests that parental expectation is a potentially powerful variable in both educational attainment and labor market outcomes. In particular, educational attainment seems to be greatly influenced by parents' perseverance in expecting their children to stay in school despite the boredom and difficulties described above, as well as by their expectation that their children return to equivalency programs after dropping out.

One variable which is related to the parental expectation variable which was not measured, but which could have important implications in labor market outcomes, is the parent's expectation that the child will earn a part-time income during high school. Final outcomes in the labor market could be positively influenced by a young person's experiences and attitudes in the part-time labor market while in high school.

Both the educational expectations and part-time employment expectations on the part of parents can be seen as related to the general level of responsibility which parents expect of their children. Early education, of course, has as one of its primary goals the creation of a positive psychological environment in the home which can support children both in school and at home. To the extent that early education does help create an environment which suggests raised expectations and a broader view of a child's potential level of responsibility, it can have a crucial impact on the final outcomes under consideration. The dynamic by which prrents come to believe that early education has changed their child's potential (it being, in large part, irrelevant whether or not a major change in potential in the sense of skills or learning style has in fact occurred), can only be hypothesized. What can be measured though, is the impact that the resulting increase in expectations (in concert with the actual increase in potential) has on the child's chances for success in school and, by extension, in final educational attainment and labor market outcomes.

Table F-1 Comparison of Six Subjects in Deutsch Sample on Selected Variables

NAME	Allan	Brenda	Carl	Denise	Earl	Frank
SEX	male	female	male	female	male	male
AGE	21	21	20	21	19	19
HIGH SCHOOL GRADUATE	yes:private (1 suspension)	no:public	no:public (1 suspension, retained 10th)	no:public (retained 7th, drops out 8th)	no:public (retained 9th, expelled)	no:public (suspensions, retained 9th)
ATTEND COLLEGE	yes: 2 yrs. @ 4 yr. college	no	no	no	no	no
ENROLLED NOW	yes	no	yes	no	no	no
PLANS TO RETURN	(in program)	yes (equiv.)	(in program)	no	no	yes (equiv.)
WANTS TO RETURN	(in program)	yes	(in program)	yes	yes (equiv.)	yes
WORKING NOW	scalping tickets	no: has young child	no: in school	no	full-time delivery man	full-time dish-washer
WORKED IN PAST	full-time salesman	no	full-time grocer,salesman	no	summer camp counselor	part-time clerk
PARENTS' BIRTHPLACE	South	South	South	no data	no data	South
PARENTS' EDUCATION	Mother has recent HS Degree	Mother has HS Degree	Parents didn't finish HS	no data	Parents didn't finish HS	Parents didn't finish HS
PARENTS' INCOME,1963	social services	social services	employment as watchman	employment as waiter	social services	social services
PARENTS' INCOME,1977	social services (tuition asst.)	employment as nurse's aid	social services	employment as admin. asst.	social services	employment as domestic
SOURCE OF SUPPORT	family	husband,mother	family	family	employment	employment
HAS OWN CHILDREN	no	1 daughter with her	2 sons not with him	no	1 daughter not with him	no
SUBJECT'S RESIDENCE	with parents	with parents	with parents	with parents	with uncle,aunt	with parents
SIBLING OUTCOME	of 4 eligible: 3 in college, 1 in army	of 1 eligible: 1 in college	of 2 eligible: 2 HS grads, in CC, nurse trng.	of 2 eligible: 2 HS dropouts	no data	no data
PARENTS' EXPECTATIONS (1963)	HS grad, lawyer	no data	Coll. grad,lawyer,engineer	HS grad, doctor	HS grad,lawyer,doctor	HS, Coll. grad, doctor,teacher
PARENTS' EXPECTATIONS (1977)	aerospace engineer	HS grad,nursing college	Coll. grad, elec.engineer	HS grad, nurse	architect,postal worker	preacher
SUBJECT'S EXPECTATIONS (1977)	not specific (not engineer)	HS grad,health professional	electrician	no plans	Coll. grad, architect	Coll. grad, lab technician
SUBJECT'S EXPECTATIONS (1979)	PhD or MS in business. Career in management	HS equiv.,clerical trng.,career in advertising	HS equiv.,vocational trng. as electician	no plans	HS equiv. or navy.Coll.trng. in computers	HS equiv., perhaps college

I.D.S. Case Studies and Salient Themes

The early intervention program which was initiated by the Institute for Developmental Studies in 1962 was designed to address the broadest possible range of factors which relate to successful coping in the urban school environment. Based on the knowledge that learning is not restricted to the school setting, an intervention program was developed which could address the mutually reinforcing roles of school, family, and community in facilitating successful school learning and in strengthening the child's self-confidence in the learning process. This program's broad scope dictated that objectives encourage not only the enhancement of essential linguistic and conceptual skills but also the development of critical social skills for coping with the demands of the school experience. To meet these objectives, both an in-school intervention program and a Parent and Community Program were established. While the in-school intervention program focused primarily on enhancing children's cognitive skills, the Parent and Community Program worked to help interested adults develop a view of themselves as teachers, and acquire broad educational skills for use in their everyday contacts with children.

A range of both objective and projective instruments had been administered to assess the impact of the I.D.S. intervention program on participating children. Children's scores on each of the instruments provided information regarding the effectiveness of the intervention program in relation to each specific area of emphasis, i.e., linguistic skills, concept attainment, etc. The broad spectrum of program objectives, however, mandated that a more wholistic view of program impact also be derived: The synthesis of school, home, and community, which was the heart of the I.D.S. program, cannot be adequately investigated through reliance [illegible] group trends in test scores alone. To provide the more wholistic perspective demanded by the broad scope of this program, case studies of individual children were developed. Here one can more readily identify the intimate, personal links among school, home and community, and begin to identify causal patterns in the interplay of social skills, academic skills, successful life coping mechanisms, and self-confidence.

Case studies of a sample of participating children were developed from data obtained during early program evaluations, plus supplementary follow-up information obtained through participant and parent interviews (with questions designed to address some of the special life circumstances confronted by minority youth in New York City), as well as a selection of additional instruments (i.e., Iowa Test of Basic Skills-Language Usage section, I.D.S. Orientation Scale-Revised form, Family Environment Scale, and Rotter Incomplete Sentences). While reporting of group trends belies the basic purpose of the case studies, several salient themes have emerged which can serve as useful guides for assimilating the information contained in them. These themes, presented briefly below, largely corroborate the viewpoint on which the I.D.S. early intervention program was based: That critical links exist among the various domains of the child's experience, and that school-related interventions must also take into account the other primary learning environments which are integral aspects of the child's identity as a learner.

Basic Themes

I. Participants viewed support institutions such as the Church and long term parent programs as having played a very central role in providing strength and direction to their families during difficult periods. Participants perceived these institutions as instrumental in encouraging and developing a more cohesive, supportive family unit which was a source of strength and encouragement to the school-age child and a facilitator of school-related accomplishments.

II. Participants who had been most successful in pursuing their educational goals indicated that the believed a significant contribution to their achievement had been made when, early in their education, an adult had taken active interest in their progress. This adult was characterized as possessing a realistic understanding of the steps required for the child to achieve dome "real world" goal or aspiration, and as providing the guidance or moral support needed by the child to approach his/her goal. The actual status or role of this adult seemed unimportant to the participants: What the child perceived as critical, however, was the experience of knowing that someone had faith in his/her ability to achieve.

III. Almost without exception, participants expressed worry and concern about completing school work and advancing in school, and reported experiencing a great deal of pressure to resolve their academic dilemmas. Correspondingly, participants frequently pointed to an academic achievement or school-related event when asked what they were most proud of in their lives.

In addition, early educational experiences with teachers who were perceived by the children to behelpful, caring and involved to provide a reservoir strength that individuals were able to draw on during some of the rough times that came later in their academic years. These early experiences as a later source of optisism or support appeared to be especially important as students entered high school. In some instances, "dropping out" or staying in school was clearly related to the presence or absence of these early support experiences.

IV. Both participants and their parents indicated that an important aspect of the early intervention program was the information that parents received in the areas of child development and educational principles. A mutually beneficial cycle appeared to develop over the course of the intervention program which increased parent's confidence in their ability to cope with children's needs, and encouraged childrens' intellectual, social and emotional development.

Following are a selection of three case studies which illustrate some of these themes.

IDS Case Study B.F.

B is an energetic young man who seems to zestfully enjoy sharing his views about the world with others. B was eager to come to the Institute to speak with a member of the staff.

B was born in New York in the fall of 1959. Both Mr. and Mrs. F had come to New York City after leaving high school in their southern home towns. Mrs. F completed 10th grade: Mr. F., 11th grade. They were both about 36 years old when B was born.

At the time of the initial interview B had three older siblings, 2 sisters, 6 and 8 years old, and a brother who was 9. The family's main source of income was from Mr. F.'s work as a watchman. He had been at the same job for over 6 years. Mrs. F. was not working. The family's apartment was located in a city housig project and was described as very clean and neat in appearance. B shared a room with his older brother, although he had his own bed. B walked and talked at about a year and a half. When asked about how much school she would like B to have in the future, Mrs. F. said, "Graduate from college". She viewed herself as a person who was "going up in the world". She believed that given the way things were going in the family, B would be able to go to college, "If he'll be able to work hard enough". Her first choice of a job for B was "Lawyer, because he asks so many questions. Otherwise, maybe an engineer".

The F family spent time together each day sharing news about what each of them had done. Mrs. F. knew that her children were doing in school and the kinds of books they were reading. She had taught B to tell time, to count, and to write his name. The family had taken trips to the zoo, visited relatives, and been outside the city for a vacation together. B's older siblings all made use of the public library. Mrs F saw to it that they had community library cards. B asked to be read to at home, and his older brother agreed to do so at least four times a week.

Mrs. F. was described by the interviewer as "extremely cooperative and very interested in the program." She was an active woman who enjoyed reading, visiting with friends and relatvies, and relaxing with TV and radio. When asked to rate how satisfied she was with different things in her life, Mrs F said she was most satisfied with the schools her children were attending,her family life, and the job her husband had. Least satisfactory were the amount of money she had to spend and the apartment they lived in.

The interviewer with the F family described B as an active, friendly child who involved himself in constructive play while she and B's mother were talking. During the first year of the program, B had two operations to help correct some medical problems. Both were successful. During his last year in the enrichment program (third grade) B's teacher became concerned that he was not working up to his potential. She noted that he was easily distracted and needed to have an adult nearby to keep him focused on his ongoing activity. In her estimation if B could attend and concentrate on his work more successfully, his reading and other basic skills would improve. A speech problem was also noted.

Fifteen years after the initial interview, B and his mother were interviewed again by the Institute. B was 19 years old, living at home with his mother and father. They were still residing at the same address. Neither Mr. nor Mrs. F were working. Their two older daughters were living on their own. No mention was made of the older son.

When Mrs. F was asked how she felt about B's progress in school, she said, "Kind of so-so, not too good." She still hoped that he would complete college, though her employment aspirations for him had changed. She now said that she would want him to be an electrician. Mrs F also noted that B's father has had an important effect on him throughout his life. She also stated that B had been a little nervous "about things in general."

Mrs. F was asked to think about the enrichment program and the effect it had on the family. What she liked most about the program was "the way they taught the kids the little things they need to know. Prepared them for kindergarten. The teachers were concerned." The services that were most helpful to Mrs. F were those that helped her to get along better with B and those that helped her teach him things herself.

When B was interviewed in 1977, he was in the 11th grade and hoped to complete some kind of vocational school. Although he was not specific about the difficulty he was having in school, he made a number of references to how things "change and get more boring [in school] as you get older."

At this point, B's progress at school was not made clear. He did perceive himself as doing a little better than others in his schoolwork and recognized that he needed to complete his education in order to get a good job. B did not participate in any special activities at school.

In his spare time, B spent time studying, looking for a job, and "hanging out." He enjoyed reading trade books about electronics and mechanics. He was reading a newspaper every day and using the community library about twice a month.

B. was working part time as a delivery boy at the time of this interview. His ability to work and make money gave him special pride. "When I was working I got my own money which allowed me to get things for myself. I knew about responsibility." B was now thinking about going into the electronics field.

When B was asked about the kind of trouble he had been in, he said, "Disruptive conduct and suspended from school." As for the person B admired most, he said, "Nobody but myself. I see how most adults struggle and have a very hard time."

B's memory about the enrichment program reflected his particular concerns about school. "I liked the way the class was separated; everyone was working on a separate level, working in groups being able to learn more. The reading program." When B was asked how school was different for him, if at all, after he left the special program, he said, "Teachers were different, the way they taught. I learned more from the groups than in the large classes." If B had a chance to implement some changes in the schools he attended, he said that he would make smaller classes and "separate the slow from the fast learners." It appears that B had problems with learning that were complicated by the large classes and by the pressure to compete and keep up with students who picked up information more rapidly than himself.

At the most recent interview, B was described as a young man with whom you could spend hours chatting about current events, sharing political views, and learning about the struggles of minority men who have had social and educational experiences similar to his. While his ideas were rich, his ability to express those ideas in verbal and written form was very poor.

At 21, B is unemployed, the father of 2 boys, 5 and 3 years old, and still working to complete his high school equivalency. B's sons live with their mother in another borough of NYC. He visits them every day. B still lives with his parents, though he spends very little time with them. Both his older sisters are married and live in his neighborhood. He sees them regularly. B's older brother, who was not mentioned at the last interview, died at about 18 years old. B was about 12 years old at the time and the circumstances surrounding his brother's death have not been openly discussed, even within the family. B's mother told him it had something to do with the police and she would not explain anything further. The family rarely talks about his brother. B has stopped asking questions because he sees the pain it causes his mother.

B described both his parents as the rulemakers in the family when the children were young, but his mother "carried it out." If the punishment was for something minor, the children were not permitted to spend time with friends and were only allowed to leave the apartment to go to school. When they were "real bad" they were hit. B said that all his older siblings stood up for him while he was growing up. He added, "They would try to guide me right." He remembers spending a few hours each week helping his grandmother in her apartment, cleaning and shopping for her.

School and education are controversial topics for B right now. The realistic attitude B had adopted comes through in his discussion. B said that his best years in school were from the fourth grade down to the time he entered. When asked why this was so, he responded, "The teachers seemed like they was more interested than the majority today. Today they just give aims..." B went on to say that once teachers give aims, they don't really give any time for explanation or discussion. If you didn't pick up the idea and never spoke up,you didn't learn. In explaining why he liked the elementary school teachers, he said, "they got down to business, they weren't boring. They didn't have attitudes until you got bad. They treated us like themselves." His junior and senior high school experiences were especially bad because he was "bored." As for the teachers, "They just tried to push you along." Math is B's favorite subject in school; English his least favorite.

B began his high school education in a vocational trade school. The vocational aspects of the curriculum "kept my mind alert -- the other classes were real boring." B worked part time to help support his family while he was attending the vocational school, and from his description, it became increasingly difficult for him to keep up with the work. Due to his poor grades, he had to repeat the 10th grade. After he completed the sophomore year for the second time, he left to enter a high school equivalency program. The equivalency program he attends is affiliated with a college, and after he completes 24 credits, he will be eligible to take college courses. B has been able to pace himself much more comfortably in this program. He has been able to contribute to his children's and parent's support and has maintained a sense of pride about his work. When asked for some of the reasons why he left school before he graduated, B emphatically said, "I didn't drop out -- I got a transfer and decided not to go to a city school and to go to an equivalency school where I wouldn't know anyone and wouldn't fool around with my friends. I had financial problems." At the time of this interview, B had only three credits left before he receives his equivalency. After that he would like to enter a vocational school in electronics or roofing. He is confident in his plans: "I'm used to using my hands and good with that stuff." When asked for his opinion of having a high school education, he said, "Not with this economy. All I think you need is a trade. But it's good to have it (high school education). I've seen people get jobs with just vocational training." B's two older sisters have both gone for some sort of educational training beyond high school. One sister is currently at a community college and the other sister is studying to be a nurse.

B's employment record is rather extensive for his age. He has had three full time jobs since he was 16 years old. In the two earlier jobs, he began as a part time worker and was then taken on full time. At the first job he worked as a salesman at a men's clothing store. Following that he worked in the produce department of a large supermarket. He liked this second job because he learned how to judge food and worked as his own boss. Unfortunately, B was sick for three weeks and laid up in bed with a bad back. When he returned, his job had been given to someone else. Within a short time, he found a job with the Parks Dept. His responsibilities included gardening and stoning. B liked learning about soil and trees -- "I'd learn how to be a doctor for the trees! I was at least learning a trade." What B didn't like about the job was the pay. "It was wrong to pay so low and do all the Parkee's job. Besides, there wasn't enough education involved." B could not continue with the job because his doctors advised him that the work was not good for his health. He had stopped working two months prior to this latest interview

B said that he was going to try to work for his brother in law in the import-export business until he could find a training program. He added, "I'd like to be working, but I'm waiting for a program -- not too much work around."

Prior to his full time work, B had at least 5 part time jobs while he was going to school. When B was asked to think about his employment aspirations for 5 or 10 years from now, he readily responded, "I'd like to go into musical engineering or making commercials. " B's understanding of how the real system works was reflected in his answer to the question, "What do you think you will really be doing 5 or 10 years from now?" "If I'm lucky, yeah"[referring to the jobs cited above] When questioned about the "lucky" he explained, "I've got to get into the right surroundings. I have to meet the right people." B said that he is primarily asking relatives to help him find work right now. He is supporting himself from savings and the financial aid he receives from school. For a short time he received unemployment compensation.

B chose "high income" and "job security" as the two most important characteristics of a job. Least important is "short working hours". When B was asked whether or not he would continue to work if he were to win the one million dollar lottery, he laughed and said, "Would stop and work for myself. I would invest money in the import-export or the stock exchange."

While he was growing up, B viewed his most serious problem to be the "bad environment up where I live." His parents, sisters and grandmother were most helpful to him. "They would tell me about what I was doing wrong." B gave an example of what he meant by the "bad environment." One afternoon when B had his sons visiting with him, a neighbor found the boys playing alone in the hallway of the apartment building. The man gave the younger son some alcohol, apparently more than his body could tolerate. By the time B got his son to the hospital, the boy had gone into what B described as a "coma". B said that he feels very thankful that his son recovered from the incident.

B stated that the only people who have been giving him a hard time lately are those who owe him money. "They don't want to pay." B added, "I don't want to be bothered with my friends who get slick." B has been in three fights with other men and has hurt them badly enough that medical attention was required. He made it clear to the interviewer that it was "just with my fists." When he was asked if he had ever hit an instructor or supervisor, B said, "No, but wanted to!"

When asked which public services he has used, B mentioned job training, employment programs, unemployment compensation and legal aid. B is registered to vote and has voted in major national and local elections.

B strongly agreed with the statements, "I try to think well of myself" and "On the whole I am satisfied with myself." While it is apparent that B is proud of himself and believes that he is a worthwhile person, it seems that he has not yet lived up to some of his personal goals. For example, B disagreed with the statement, "I feel that I have a number of good qualities" and emphatically added, "Not the way I want it." Until he achieves his goals, he will not have the kind of self-respect that he looks forward to. B is proud of his educational achievement and self-motivation. "I feel proud about getting into school and the college. If I really want to do something I can do it." B made an interesting comment about doing volunteer work: "On my time I used to help old and blind people. Not anymore because people think you're going to rob them."

What would be a"really good time"for B? "Being around a good environment. Nobody cussing or calling each other nigger or bastard or ripping anybody off. Listening to music and partying." For the two most important areas of life, B chose "good health and good family life." He said that he really couldn't exclude "having money" as important because it's "the source of survival." Least important to him were "hobbies" and "having good friends and the right number of friends."

When B was asked to describe a typical day, he said, "Thinking about my future every day." He said that he discusses what he does during the day with a few of his friends "just to feel relief."

B's responses to the incomplete sentences reflect a richness of thought, but a severe difficulty with verbal and written expression. The Institute staff decided to include his entire record because we were so touched and struck by his responses. One can imagine how much easier it would be for B to achieve his goals if his skills were more well developed.

The happiest time of life
I regret not to fine school
I feel that i am go to be rich someday
In school we all look for a fruture
I can't feel the same way a be life
When I was a child i play baseball all day
Reading is a subject i have Bad time in
I need my GED of my fruture
I wish that the world would leave to love
I will be somebody some day
My greatest worry is leaveing school
I am best when play baseball or working on a job
Sometimes my mind think of way of make it in the world

B scored below the average grade level of his peer group on the language skills test. He scored about average on the current orientation scale. B's perception of his family gives the Institute the picture of a cohesive family in which achievement in employment and involvement in intellectual activities are stressed.

I.D.S. Case Study W.H.

WHJ is an attractive young black woman who celebrated her twenty-first birthday this year. When an Institute staff member tried to contact W to ask if she would be willing to come in for another interview, her mother told the staff member that W was living in the Mid-West and would be back for a visit during the summertime. We were able to contact W when she got back to the city and she came in for the interview with her own daughter, who is now four years old.

The Institute staff first met W's family in 1963 when they applied to enroll W in the IDS pre-school enrichment program. Mrs. H kept in close contact with the Institute throughout W's experience in the school program. She and W were then interviewed on separate occasions in January, 1978. This most recent interview with W was one and one-half years later in July, 1979.

Mrs. H and W were first seen together in their apartment in West Harlem just prior to the inception of the enrichment program. W would be amongst the first group of children to enter the pre-school enrichment classes. At that time, Mrs. H and her husband had just separated. Mrs. H, her seven year old daughter, S, and W shared a one bedroom apartment. There is no information about the condition of the apartment available from this initial meeting.

Mrs. H was described by the interviewer as being fairly involved and interested in W's possible acceptance into the enrichment program. Mrs. H described W as an active child who walked and talked when she was about a year old. She had no significant health problems and was a fairly independent little girl. W had never attended any other pre-school programs.

Mrs. H had graduated from High School in her Southern hometown. There was no information available about W's father's education. At the time of this pre-school interview, Mrs. H was unemployed and the family's sole income was welfare. Mrs. H made the rules for the family and reported that her primary means of punishing the children was to take privileges away from them.

Mrs. H and W were not interviewed by the Institute staff until 1978. When we met Mrs. H and W again, the family was living in a public housing project for low and moderate income families in East Harlem. W was now nineteen years old, recently married, and had a three year old daughter. Mrs. H was working full-time as a nurse's aid at a Senior Citizen's home. W's sister was in her first year of college.

At this follow-up interview with W and her mother, W spoke very little about her relationship with her daughter or her husband. Her husband was in the Air Force, stationed at a base in the Mid-West. W and her daughter had lived there for a few months, but returned to live with her mother. She was attending the eleventh grade at a special program at one of the colleges of the City University and hoped to get her diploma and complete college. Math and English were her two best subjects in school. She said that she was not doing as well as other students, academically. She hoped that her education would help her get a job in the health field, specifically in maternal and prenatal care.

W had not been involved in any extra-curricular activities at school and spent most of her free time taking care of her daughter. W stated that she spent about 20 hours a week reading books and magazines, and that she used the community library at least three times a month. Approximately 2-3 hours a day were spent watching T.V.

When W was asked what she was most proud of, she said that it was entering the equivalency program. She most admired "Professor M.H. because she can deal with problems, doesn't show any weakness."

Mrs. H spoke highly of W's accomplishments in school. Although she had left high school after her pregnancy, Mrs. H felt that she did a wonderful job for the three years she did attend. Mrs. H said that W's grades were good and she hoped that her daughter would finish regular high school. She wanted W to be able to go on to college. Mrs. H felt that with the current family situation

W would be able to get some college behind her. Mrs. H hoped that W would become a nurse.

Mrs. H was asked to think back to the time when W was in the pre-school program and what she liked best about it. She said "Reading and writing - also it helped her get along with the other children." Mrs. H was most pleased with the help she received in learning how to work along with W. "I learned how to cope with her and we got along very well." Mrs. H also added that she wished there was such a program for her granddaughter, W's daughter.

When W was asked about how the family generally got along, she said "about average." The family usually ate dinner together and talked about their day's activities. An observer during the interview commented on how well Mrs. H and W got along. There appeared to be greater tension between Mrs. H and her older daughter.

After the interview in 1978, W and her daughter returned to the Mid-West Air Force Base so the family would be together. Although W enjoyed the life there, after a year and a half she and her daughter, once again, returned to New York City. W had carefully planned this move and had set clear time limits on their stay. W moved back to N.Y. for one year so her daughter would be able to go to kindergarten. In the Mid-West, she would have had to wait a year. W said that she was very anxious for her daughter to start school, and this would be a way for her to get started in the first grade in the Mid-West. Once her daughter entered school, W would have more time to finish her own high school education.

At this most recent interview, it was learned that W had moved back into her mother and sister's apartment. When questioned about the family circumstances while she was growing up, W described a very warm, supportive environment that was largely created as a result of her mother's efforts. After her parents' separation, W rarely saw her father. She believed that she had a half brother, but she was not sure. Her father died when she was eighteen years old.

W clarified some of her personal history. W had become pregnant in the tenth grade and dropped out of H.S. just before delivery. She and her boyfriend did not want to marry until he was more settled with a career or could support a family. W returned twice to H.S. "because I needed to", but found it too difficult to manage everything. Her grades at school were good and she was never suspended or expelled. Once her boyfriend was settled in the Air Force, they got married. W and her daughter then joined him in the Mid-West. Despite the interruption in her education, W still believes that she will get her equivalency diploma and then go on to a two year college. (W signed up to take the equivalency test about one month after the interview.) She believes that a high school education is a requirement for any job.

W has never had a paid full or part-time job. When asked why she was not working at the present time, she replied, "My skills, I'm unskilled and I have no babysitter." W would be starting a clerical skills training program as soon as her daughter started school. Ultimately she hopes to get an advertising job in the art world. W was able to project 5 or 10 years into the future and optimistically talked about how she could then finally achieve what she set out to do. She stated that the most important aspect of a job was the chance for advancement or promotion. Least important to her were short hours and free time. When asked what she would do if she won the one million dollar lottery, she said she would continue to work: "I need to work for self-fulfillment and achievement." When the interviewer asked her whether she was planning to have more children, W said, "No more now, I have things to do at the moment."

When W looked back over her life, she said that her most difficult problems had to do with getting into fights in her neighborhood. Now, her most serious problems are getting skills and dealing with administrators at the Public Assistance office. W said that she was most proud of the fact that she had been given some jobs that she had applied for, although she did not end up taking them.

She again mentioned that she was proud that she had entered the G.E.D. program in 1977, even though she never completed it. W was presented with a list of values in life and stated that the 2 most important to her were to have an interesting job and a good family life. Least important were having good friends and hobbies.

W had used a number of agencies in the last few years, including CETA, A.D.C., employment services, and family planning clinics. She has done volunteer work on and off at a community church in Harlem. W is not registered to vote and has never voted in an election.

W's projective statements reflect her current concern with the need to follow through with her education. W stated that the happiest time in her life was when she got married. She responded to "I regret" with "the time when I didn't return to school." Her concern with developing skills is clear, "Reading is very important, tell the young." Finally, "I wish I was in college."

When W left the interview she asked the staff member to please keep her in mind for any future interviews. She also agreed to help coordinate a conference of former IDS participants.

When compared with her peers, W fell below average on the Language Skills Achievement test, though above average on the current Orientation Scale.

W's interview took approximately two hours, after which the interviewer and W chatted more about the differences between her life in Harlem and out in the suburban Mid-West. W feels that life in the Mid-West is much calmer and, in general, "better for your health." During the interview, W's daughter sat with crayons and paper, drawing and entertaining herself. At one point, when D became lonely for her mother's attention, she very politely asked to sit on her mother's lap for a few minutes. She gave her mother a big hug and then went back to her play.

I.D.S Case Study U.G.

U.G. has had the special gift of gab since his early childhood. As a 21 year old young adult, U is a poised, charming fellow. His capacity for creative thinking and self-understanding is helping him to create the kind of life style he hopes for. U's intelligence and charm can be very impressive. For example, on the day of his most recent interview with the Institute, U did not appear at the appointment time. One hour later, the interviewer received a call from U that he had not forgotten the appointment but had been called in for a job interview that had to take preference. He asked if he could come in later in the afternoon. When he arrived, he shook the interviewer's hand firmly , smiled, winked, and said, "I apologize for being late, I do hate to keep a lady waiting."

U was interviewed three times over the last 17 years, his mother twice. The first interview occurred in 1963, a few months after U entered the enrichment program. Mrs G and U were then interviewed on separate occasions in 1977 when U was 19 years old. The last interview with U was in July 1979.

U was born in New York City to a 22 year old mother. U was the fourth son of seven children who were living with their mother in a West Harlem apartment. At the time, Mr. G was not living at home, though he was responsible for the support of the three youngest daughters. Mrs. G was unemployed and the primary support for herself and her four sons was welfare.

U was described by his mother to be a very active child who was always wanting to know "Why?" There is no information available about his developmental milestones. U had no unusual childhood health problems.

Mrs. G was born in Georgia, U's father in Virginia. Mrs G completed 8 years of elementary school and then spent 1 1/2 years attending secretarial night school. U's father finished high school and then received training with a business agency. Despite Mrs G's busy life with her seven children, ages 1-9, she found time to read books and magazines, visit with her friends and relatives, and devote some individual time to each of her children.

The G's apartment at this time was in a low income,city housing project. Each child shared a bed with his or her younger sibling of the same sex. The interviewer rated the apartment as being very badly run down with insufficient lighting and broken windows. The apartment was drab and very messy. The neighborhood was described as being moderately kept up.

Mrs. G. devoted a great deal of time and energy preparing her children to enter shool. Mrs. G had gotten her two oldest sons library cards and made sure they read to U. During school, Mrs G or one of the older sons read to U every night. Mrs G. had tried to teach U such things as how to write his name, to count, and to read the letters of the alphabet and short signs. Even at this early stage, Mrs G. expected U to do well in school. Mrs. G said that she used "the strap" and took away privileges when she punished the children. Although Mrs G did not speak about the importance of her religious faith at this time, this variable, as we later learned, was a source of cohesive strength for the family.

Mrs G perceived herself to be most like people who were "going up in the world." She hoped that U would ultimately be able to go to college and become a lawyer. However, when asked about what she believed his education would really be like given their current family situation, she said that she was sure that he would at least graduate from high school.

Although this interview occurred only a few months after the preschool program began, Mrs G said that she had already noticed changes in U. "He's more of a little man and he seems to help himself more."

Young Adulthood

The next meeting with U and Mrs G revealed that her perception some 15 years ago about where she was going in life was correct. The G's were now living in a larger apartment, more suited to the family size. This was still in a low income project. The oldest son had moved out and enlisted in the Armed Services.

Since the initial interview, Mrs G remarried and had two children with her new husband. However Mrs G and her new husband were having marital difficulties and were living apart. Mrs. G had gone back to finish high school. At the time of the 1977 interview, she had completed her first semester of college. U had graduated from high school and had also completed his first semester of college.

Mrs G's pride in U's educational accomplishments is best captured by her own words: "I think he has done great wonders." She hoped that he would ultimately have the job he preferred, to be an aerospace engineer.

Ms G was very pleased with U's experience in the enrichment program. She appreciated the manner in which the teachers integrated practical experiences with the theories of education. She remembered most vividly the math workshops that were given for the children. She was also impressed with the special family services that the program offered. For example, from home economics workshops she learned household management and how to "stretch money." Mrs. G's additional comment indicates how widespread the impact of a child's involvement in a preschool program can be. "I learned the concept that girls as well as boys learn." It is important to note that one of U's older brothers entered a college program shortly after U. The two oldest daughters have also entered college programs, one in Philadelphia, one in New York. Mrs G also stated that an important aspect of the program for her was that she received help in learning how to teach her children things herself.

U had moved up through the NYC educational system in a different manner from most of his former classmates. The guidance counselor who worked with U during his second and third grade classes while he was part of the enrichment program arranged for him to take special tests that would qualify him for scholarships to a private Junior High School. After U excelled at the Jr. High, he went on to attend a private religious High School.

When U looked back over his school experiences, he stated that he liked the strict rules and values taught at the Jr. and Sr. Highs. Math and science were his favorite subjects. U, however, disliked taking religion courses and "dealing with the Brothers, the people who were supposedly enlightened by God." U believed that he had done better than others, academically, in his classes. In high school, he took part in sports, social clubs, and the Afro-American Club.

In U's free time he usually studied, went to the movies, or spent time with his friends. Reading was his favorite pastime; novels, autobiographies, and magazines were among U's favorites. He stated that he used both his community and school libraries.

U had not worked within the year prior to the 1977 interview. He was no longer interested in becoming an aerospace engineer.

At the most recent interview, U spoke very clearly about the difficulty he was having in school. He said that the second year of college really shook his confidence, so he took a year off. During that time he worked as a salesman in a department store. With encouragement from his family and some time to recover, he returned to school.

When U looked back, he stated that he realized how important his early years and Jr. high experiences were. "You had to give your best effort." He spoke fondly of his second and third grade teacher (a teacher associated with the enrichment program), "She had a real subtle, frank approach. She admired me and still pushed me." As for his attending college, U said that his family pushed and it was expected of him. During his high school years U was president of the Afro American club, president of the Chess Club, and captain of the basketball team.

In the two years since the last interview, U changed his major and decided to get a B.S. in Business. Ultimately, he hoped to get a Doctorate in Business. When asked how far he believes he will really be able to go, he said, "I'll get at least a Master's." U's pleasure in learning is clear; "I'm fascinated by all that knowing." The reason why he might not complete as much school as he would like to, as he sees it, is because he may get married and get tied down with a family. During H.S., U was suspended once in his senior year for visiting another school with his friends. U then served detention for a few months and the suspension had no effect on his graduation.

When U was asked about the importance of having a H.S. education, he said, "Yes, it's essential. In America, without a H.S. diploma you won't get a trade. You need it to be at least middle class." About the importance of a college education, "I think a college ed. prepares a person for really asserting themselves in society -- but it depends upon your choice."

At the time of the interview, U was neither working full or part time. U had worked within the year while he was on his leave of absence from college. As a salesman he liked the challenge of meeting people and "finding subtle ways of influencing them." He did not like to do the stockroom work. He had also worked part-time as a door to door salesman. Although he said that he would prefer to be working, he had decided to take a little vacation from looking. When he finally decided to search seriously, the market was already tight. U was, however, making money on the side by scalping tickets for things like sports events and concerts. Although he appeared to be embarrased sharing this information with the interviewer, he was most clearly upset when he talked about his mother's disapproval of his activities. U viewed this as a convenient way to make money, money that he needed to have while he was in school. As for his future employment aspirations, U said, "I'd like to be a president of a company handling accounts and marketing operation." U said that he didn't really believe that he would ever be the president of a company, but rather that he would end up working for someone else in a segment of an organization. "Because from experience, I've seen that I'm a dreamer and my ideals, well, I'll settle for less." For him, chance for advancement or promotion are most important in a job. Least important for him in a job is job security.

U's pride in his volunteer work seems to be an important aspect of his self-image right now. He has volunteered in hospitals and has worked with children and senior citizens. Most important to him was his work with a community education program in which he worked to "help kids figure out study methods around all the chaos."

When U was asked what he believed to be his most serious problems when he was growing up, he said, "My step-father, he's got problems. I don't know why my mother married him." He believes that one particular friend and his brothers played important roles in his development. Of his friend who tutored him in the community education program, he said, "He gave me a freer attitude toward people. He taught me it's the right frame of mind and not necessarily the money to help you get ahead." His brother "set serious model types and taught me you don't have to be a part of all the daily problems, but also don't close your eyes to them." U has learned a great deal from these role models in his life.

Personally, U would like to get married and have 5 or 6 children. Most important to him are having a good family life and an interesting job. Less important to him are having a strong religious faith and having good friends and the right number of friends.

U spoke at length about the role his mother has played in setting limits for her children and in fostering a strong family commitment to each other and to community members. U believes that his mother's rule about family attendance at church has had one of the greatest impacts on their family cohesiveness. His mother also set a rule that she would only continue to provide a home for her children so long as they stayed in school or worked. Every Sunday, the family goes to church together -- "You go even if you're not religious." Once her children reach 17 years of age, they can make their own decisions about church attendance and education. Although U has decided that religious affiliation is not important to him, he recognizes how significant attending church together as a family is. He also recognizes how important the kind of support that religion gives his mother has been to his maintaining the kind of motivation he has, despite the financial difficulties of his family.

U's projective statements reflect his strong motivation to do well in life, to participate as a concerned member of his community, and to live up to his high personal standards. While he is able to exert strong control over his educational and work activities, he is also able to let his imagination flow freely. The following selected statements capture some of his perceived strengths and weaknesses and current concerns:

I regret taking leave of absents [absence] from college.

I feel that everyone has an obligation to be the best they can be.

I need to be more assertive in my endeavors.

My greatest worry is that I will fall short of my ability.

I love being cared about and talking to people about their problems.

I am best when under the greatest amount of pressure.

Sometimes I like to get away and fantasize.

U's scores on the language skills test and on the Orientation scale fell above the average score of that of his peer group.

(12/1/79)

Attachment D

Instruments

INSTRUCTIONS FOR 1979 SCHOOL DATA INSTRUMENT

General Instructions

The 1979 School Information Form has three parts: Part A is School Data Form (with instructions) to be collected for all individuals where possible; Part B includes supplementary questions for school folder of underachievers (students placed in special classes, and/or retained, and/or dropped out of school); Part C is suggested interview with school staff to ascertain official (and operating) school policies, guidelines, and criteria concerning special education, retention, expulsions.

1. All coding boxes except those marked "FOR OFFICE USE", must be filled in. DO NOT LEAVE BLANK BOXES. If there is no information; or question is not applicable, or don't know, record 9, 99, 999 (for 1, 2, and 3 digit codes).

2. Write legibly. If your answers are not legible, they cannot be coded. This is a precoded form. You record the code number in boxes. The numbers next to the boxes are the numbers of the columns on computer cards. The computer cards will be keypunched directly from these forms, so that it is essential that the numbers are entered in the box in a legible manner. Do not write in the "OFFICE USE ONLY" box and do not write in the keypunch area as it will be confusing to the keypuncher. Use leading 0 for one digit numbers in 2 box codes as shown on the form (e.g., February, 1972 is recorded 02, 72).

3. The forms are printed on carbonless paper. When the School Data Form goes back to the project office, it will be taken apart and the white originals stapled together to be sent to Cornell with the individual project retaining the copy for safety reasons. The forms will be coded and keypunched at Cornell and a duplicate set of cards returned to the project. Important: insert the cardboard behind the colored carbon page so that the response is not inadvertently imprinted on the subsequent pages. Use a ball point pen and press firmly. If you made a mistake in the recording, instead of writing over the incorrect number, use white out liquid and then make the correction. Do not use correction tape because it peels off too easily. You can make the corrections neatly and carefully back at your office.

4. Please send the first 5 forms to Cornell when they are completed so that we can find any inconsistencies or ambiguities.

5. Important: In order to preserve the validity of the data, the person who records the data should be blind to the research questions being asked. The child ID codes should be carefully guarded, and whatever method the project supervisor can devise to guard against these biases would be worthwhile.

6. Note. The School Data Form and the individual school records will not necessarily coincide in sequence; therefore, it might be useful to skim the school records prior to filling out the Data Form.

7. Schools or communities may offer special local programs with various purposes. You should feel free to record as much information as possible to explain the purpose of the program. For example, if you specify a special program in #6 (Part A) like "Youth Development Treatment Center", explain in #23 why R was referred or the general purpose of the treatment program for R.

Instructions for School Data Form - Part A

This instrument was designed to gather detail on the school careers of Consortium young people. Part A should be completed for every subject.

Carbonless paper is used so the recorder must insert a piece of cardboard under the colored sheet for each page of the form so that marks will not penetrate beyond the copy. Remember to write legibly.

Note. 9, 99, 999 indicate no information, not applicable, don't know. DO NOT LEAVE ANYTHING BLANK.

At the top of the page, enter your project ID number and the child ID number. Instructions for each question follow below.

#1 Normal progression is indicated as a guide.

#2 Record year (e.g., 1962-63) beginning with year subject started (or would have started) Kindergarten. Fill in all relevant consecutive years to present (or until out of blanks). Do not record any calendar year more than once.

#3 The first row of boxes for subject grade level is for kindergarten (col. 9-10). If attended kindergarten, record 50 in the first row (col. 9-10). If did not attend kindergarten, (or there is no information for kindergarten), record 99 in first row of boxes (col. 9-10). Then proceed down the category, recording each grade level (e.g., 01, 02...12). If a grade is repeated, record the same grade twice (e.g., if repeated 3rd grade, record 03 for 1965-66 and 03 again for 1966-67). If in ungraded class, record 21. Use 21 with special education (e.g., for special ed classes or Karnes' "storefront").

For students who graduated, enter 99 in all subsequent boxes. DO NOT LEAVE BLANKS. Do not include post-high school training in #3. Record post-high school training information in #13.

For students who dropped out of school, record the last grade level s/he completed and got credit for next to that year. If s/he started the following year but did not complete the year and received no or partial credit, record 20. Then drop down to the next row of boxes and record 22 (for dropout) and enter 99 in all subsequent boxes. Code 22 identifies this student as a dropout (never returned to school). For actual date of dropout, see question #14e. Code 20 can mean incomplete year (student dropping out); code 20 can also mean an incomplete year in which the student has not formally dropped out and the school is still carrying him on their roles. Code 20 is always used in a negative sense of the student no longer attending school.

If S moved out of the district and came back, and the progression is not known for the missing years, record 9 for no information in #3 through #8 and explain what happened under #11.

#4 This refers to the decision made at the end of the school year. Record 1 for regular promotion; 2 for social promotion; 3 for retained; 4 for ungraded; 5 for skipped a whole or part of the year; 6 for partial credit upon returning to school; 7 for retained for part of a year; and 9 for no information.

If completed 12th grade satisfactorily, record 1. If presently in 12th grade, record 9. Record 9 for current grade. That is, if in 1978-79 child is in 10th grade and the school year hasn't been completed, record 9.

If records show child had normal progression, assume "regular promotion" (1) unless "social promotion" (2) is specified in the records.

If child was retained in grade, but entered the next grade on schedule because s/he went to summer school: code #4 as 3 = retained, code #6 as 2 yes, specify summer school, code #10 as 2 = yes and code #10a and b as you normally would. If a child, for example, was retained in 8th grade but repeated only the first marking period, code 8th grade as 7 = retained for part of a year and show the progression into 9th grade since that is where he spent the majority of the school year. Code #10 as you would for any year in which there has been retention.

When #3 is coded 22, #4 will always be coded 9.

If after dropping out, the child goes back to school in the middle of the year but doesn't, as a result, have enough credits, for example, to graduate, code #4 as 6 = partial credit upon returning to school. Code 6 is used in a positive situation where the child is returning to school. Code 6 is never used in #4 when #3 has been coded 20. Code 20 is for leaving school (negative) whereas code 6 is for returning to school (positive).

In unusual situations where the child, for example, has been retained for part of a year, has skipped a year, has been promoted after attending summer school, has dropped out and come back, give the details under #11.

Examples:

#3	#4
05	9
06	1

Child retained in the 5th grade for first marking period. The child was placed in the 6th grade after repeating one marking period of 5th grade.

#3	#4	#6	
08	3	2	summer school
09	1	9	

Child retained in the 8th grade, but followed normal progression after attending summer school.

	#3	#4
75-76	11	3
76-77	20	9
77-78	22	9

Child retained in 11th grade. Did not complete the repeated year, but dropped out 3-77.

	#3	#4
74-75	10	1
75-76	20	9
76-77	11	6
77-78	20	9
78-79	22	9
79-80	99	9

Child promoted to 11th grade, but due to pregnancy stopped attending in 10-75. Child then returned to school in 1-77 and received partial credit for an incomplete year, but dropped a second and final time in 11-78.

	#3	#4
76-77	11	1
77-78	20	9
78-79	12	6

Child did not complete the 12th grade. Child dropped out 11-77. Child then returned to school in 2-79 but did not have enough credits to graduate at the end of the school year.

#5 Record 01 = regular class, 02 = special education (unspecified)*, 03 = EMR or EMH (Educable Mentally Retarded or Handicapped), 04 = TMR or TMH (Trainable Mentally Retarded or Handicapped), 05 = ED or BD (Emotionally Disturbed or Behavior Disorder), 06 = LD (Learning Disability), 07 = PH (Perceptually Handicapped or Impaired), 08 = Gifted, 09 = other special classes (specify on line provided), 10 = S/H Speech and Hearing only. If received speech and hearing in addition to other special classes, record 12 for S/H plus special education (unspecified), 13 for S/H plus EMR, 14 for S/H plus TMR, 15 for S/H plus ED, 16 for S/H plus LD, 17 for S/H plus PH.

*Note: Use code 02 only as a last resort. If, for example, neurologically impaired class is indicated, record 09 = other and specify -- it should not be 02 = unspecified. If other, see additional codes on office codes sheet.

Use code 01 for regular class placement, use code 99 only where you have no information.

#5a Some school records indicate if the special class replaces entirely or supplements regular class. With mainstreaming, we may see more special education which supplements regular classes. Record 1 for replace; for students who required special education in all areas of study (i.e., self-contained special education). This student was not able to progress at a level with students in a regular program, and was therefore placed in a class where individualized instruction took place.

If the special education class supplements regular class, record 2 for integrated (mainstream) special education. This refers to a student who required special education in some but not all areas of study. The student was able to progress on a level with students in a regular program in some areas, but in other areas, the student required more concentrated help and needed the services of a teacher of special education.

Record 3 for institutionalized care, if a student required such specialized care that s/he was taken out of public school and placed in a residential institution (not correctional facility). However, if student is in correctional facility, record 4.

If you will not be using the detailed information form (School Data Form - Part B), use the lines in #23 to describe the type of special teacher or special class and the percentage of time which student spent out of the regular classroom.

There may be some confusion with this coding. You should record 1 (replaces totally) if 100% of time (30 hours per week) is spent in a class other than regular class. It means that the type of class experience totally replaces the regular classroom. You should record 2 (supplements) if a special class teacher supplements the regular class. For example, if child spends 5 hours a week in learning disabled classroom, you would record 2 in #5a on Part A and then on Part B you would record 05 in #9c and 083% in #9d (25/30 hours).

#6 If the school record indicates that a student participated in other special programs, record 2 and specify program, otherwise record 1. Example of special programs are: summer school, summer camp, Upward Bound, Homebound, college outreach, advanced placement, enrichment programs (art, music, summer, weekend) open classroom, special language class). If special tutoring is "less serious," (e.g., remedial reading, Title 1 reading or math) indicate participation in #6; if problem is "more serious" record in #5. If yes, see additional codes on office codes sheet.

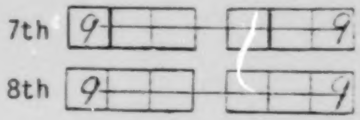
#7 In elementary schools, students may or may not be assigned to ability groups or tracks. If there is evidence of ability grouping, record this information here, i.e. Track 1 for slow or lower ability group, Track 2 for medium or average track, and Track 3 for above average or highest track. Even if in special classes record track if indicated.

For high school: record 4 for vocational/technical, record 5 for regular or general course, record 6 for college preparatory or academic program. If student is taking classes in several tracks, record the highest track (e.g., if only honors math, record 6).

Use codes 4, 5 & 6 for grades 7-12 (Jr./Sr. H.S.). If your school uses slow, medium and high tracks in 7th and 8th grades, use codes 1, 2 & 3 instead of 4, 5 & 6.

#8, #8a Schools may distort attendance records, but the distortion is probably random throughout the class lists. We are only looking for relatively high versus relatively low absenteeism within a school system. Record number of days absent for the year in #8. For example, 08 = 8 days, 00 = not absent any days. If no information, record 99. If absent over 97 days, record 97 in the boxes in #8 and indicate the actual number of days in #8a. Describe a circumstances for lengthy absences in #8a on page 2. Consider a "lengthy absence" to be about 2 continuous weeks or about 50 days. If no reason, please indicate that.

#9,#9a Some schools (NYC) indicate class rank. Record rank in #9 and class size in #9a. (E.g., if 6th in class of 125: 006, 125.) If information is missing on either rank or class size, record 999. If you are not recording any class rank, you may record a 9 in the first box on the left and draw a neat line across the boxes. For example:



If the school is very large and the class size is over 998 students, record 997 in class size boxes (col. 34-36) and record class size to the left of the #9a boxes. If student has a class rank below 998, record 997 in class rank boxes and write actual class rank to left of grade level.

#10,#11 Retention information should agree with #3. If there are comments or unusual circumstances mentioned, describe in #11.

#10 In some cases, a student may fail every course in 12th grade, for example, and thus be required to do 12th grade work again the following year. If s/he comes back and repeats the grade, code #10a as 1 and #10b as 12. If, however, s/he comes back in the fall and starts to repeat 12th grade but leaves school before completing 12th grade, you still code #10a as 1 and #10b as 12.

If #10 is "no", (never retained), then record 9 and 99's in #10a and #10b.

#10b Grades "repeated" is ambiguous. What we want to know is which grades were completed unsatisfactorily. That is, which grades were failed. Include partial year.

#12 If child is still in school, you must record 9 in column 77.

#13a If there is an indication that student is in a high school equivalency program, record 20 in #13. If in ungraded class, record 21 in #13a. 98 = moved or don't know.

#13b If respondent is in post-H.S. program, specify on the line provided and leave column 80 blank so it can be office coded.

#14-#16 Record date of dropout, suspensions, expulsions and re-entries where applicable. Some school records (e.g., Gray) indicate reason for these events.

#14 High school dropout means left school without graduating. If student withdrew from school because s/he moved to another district, that is not a dropout; record the fact that S moved in #11.

If #14 is no, then #14a through #14e should be coded as 99.

If #14 is yes, but subject returned and graduated or is presently in high school, code 14e as 99; 14e is always 99 unless subject dropped out of school before graduating. For example, if subject dropped out but returned and graduated, code 14e as 99. If, however, subject returned and then dropped out and never graduated, code grade level that he completed in #14e.

#18 Record 2 = yes, if there is an indication in the folder that the school or parent felt the student should be evaluated. Because of new legislation (PL94-142, the Education for All Handicapped Children Act of 1975) some students who are still in school may have an Individualized Education Program (IEP). Record brief comments or describe circumstances in #18 if you are not recording detailed information in School Data Form Part B. If there is no indication of a referral for school problems, record no = 1; if no information, record 9. Use the lines provided in #23 if needed.

#19a Calculate total number of schools attended by simply counting the number including changes within the year. For example, record 06 if attended 3 elementary school, 2 junior highs, and 1 senior high school. Do not include prekindergarten or post 12th grade years.

#19b Some schools keep complete records from kindergarten, other records are not complete. If you are able to record only the number of secondary schools, for example, record 1 = no in #19b and specify the grade levels that are included. For example, "information available for grades 9-12 only." Record 9 for no information.

#20,21 Older subjects may have SAT or PSAT or ACT scores included in their folders if they considered attending college. They may also have copies of letter of reference or transcript requests. Project sites with younger subjects should record 9 = not applicable. After 10th grade record 1.

Optional Information:

#22 Many schools now require Basic Competency Testing, usually at 9th grade. If you want to collect these scores, use the 1977 Achievement Test Forms. Notify Cornell and we will send you the forms.

This question tells us whether or not Basic Competency tests were given. If the test was given and the score was not recorded on the Achievement Test Form, then the test and grade level should be recorded on the line provided. If the child is in 9th or a lower grade, i.e., too young for the test, record 9 in the box.

#23 Another interesting point, Karnes' staff found that some kids' folders name the preschool attended on the face sheet under "schools attended". It is really important to see if "preschool attendance" is recorded on the school folder of other project sites. Please indicate that on page 4, #23, Part A.

#24 Suggested Procedures for Recording Course Marks

ACYF suggested that we collect grades. Course marks, however, are full of reliability and validity problems. Nonetheless, grades indicate how a student is doing in his/her own setting to students, peers, family, and other teachers. Every school gives marks to indicate achievement or academic progress. They may be based on a scale of letters, numbers, words, phrases, or percentages. The enclosed procedures are designed to permit conversion of all marking schemes to a common mark value: on a scale of 5 to 1. This scheme (or a similar scheme) was used previously for 1976-77 Consortium School Record - Page 2 Supplement by Gray's, Beller's and Karnes' projects.

Whether grades for every year or selected years are obtained will be a project decision. It does not seem to be necessary to collect grades for every year, so space has been provided for 5 years of grades. You might want to choose transition years such as 3rd, 5th, 7th, 9th, and 11th. At the minimum, collect 4th grade and the latest grade.

If the school gives letter grades, record the appropriate number from the follow code:

5	A	(excellent, superior, outstanding)
4	B	(above average or good)
3	C	(average)
2	D	(below average)
1	F,E or U	(failing, poor or unsatisfactory)
7	S	(satisfactory or passing)
9		(no information)

Some schools do not give grades, e.g., in the primary unit. Try to fit the verbal comments of the teacher into the above code for each subject. Comments such as "child tries hard" or "works to capacity" are difficult to code. If possible, ask the school staff person like the principal for help in fitting these verbal evaluations of student progress into the above code. If impossible to code or subjects are not offered, record 9.

If school gives numerical grades, use the school key to convert them to a letter grade and then record using the above code. For example, the school key might say a reading grade of 70-78 is a D, which you then record as 2 in #24a. If there is not a school key to convert the numerical grade to a letter grade, ask someone at the school if possible to transform their numerical grades to our coding system. It is particularly important to define failing scores or marks.

#24abc Mark Value for Course. For all three subjects (reading, math, science), the first choice is to report the average letter mark for the year. If the mark reported is for part of the year only, i.e., one semester, record that mark. Use 9 for no course.

#24a If there is more than one course related to reading, choose reading (first choice), language arts (second choice), or English (third choice). In secondary school, choose English over literature courses if both are listed.

#24bc If more than one math or science course is listed, choose the one that has more academic focus. For example, choose chemistry over health. If both chemistry and physics are taken the same year, record the course in which the better grade was received.

#24def The purpose of these questions is to obtain an overall impression or rough estimate of the student's work. Is the report card good or below average in general? The report card evaluation is in the context of the student's classmates and community.

#24d Record the total number of failing marks for the year in all subjects including study hall, gym, art, band, etc., whatever the school gives a mark for. However, disregard courses which give less than full credit, e.g., some courses may be listed as 1/2 credit. Some schools give a mark for "effort" shown in each course. Do not include this "effort" mark when totaling the number of failing marks. Total number includes both semesters or all marking periods. Exclude study hall, art, etc. if you wish, but be consistent in #24d and #24e. If no failing mark, record 00.

#24e Total the number of courses the student took, including gym, study hall, etc., as in #24d. The idea is to get a rough estimate or impression of how much of the student's day was spent receiving unsatisfactory marks.

#24f Overall Impression or GPA for Year. This is meant to be a relatively subjective impression by the field worker. The field worker must be blind to the experimental or control status of the student in order to record this data. A five point scale is suggested: excellent, good, average, below average, and poor. We have not been able to invent a definition that will consider the grading scheme of every possible school. You may use the procedure suggested below or whatever definition your project feels appropriate. The measurement error is obviously large in any scheme so be innovative. Several possible procedures are described below.

- If a yearly numerical average is given, use the code below directly. For example, a year average of 86.5 = 4 in the coding system. Use this code even if the school used a scheme different from ours for computing this figure. Disregard the fact that the school may compute this year average by ignoring study hall, for example, whereas in our coding system, study hall would be included in the total.

Record the GPA or overall grade according to this code:

5 Excellent; mostly A's; in 90's; teacher evaluation mostly superior
4 Above average; mostly B's; in 80's; teacher comments mostly good
3 Average; mostly C's; in 70's; comments mostly average
2 Below average; mostly D's
1 F's, U's; comments poor

7 Satisfactory
8 Too variable to code in above
9 No information

Use code 8 (variable report card) if you feel the report card is too variable, spotty, or irregular to classify as 1, 2, 3, 4 or 5.

- If an overall grade point average (GPA) or numerical average is not given, the following procedure can be used to determine the GPA. This procedure tallies up all the grades for the year including deportment, physical education, art, shop, study hall, etc. using the 5 - 1 code listed above. Consider an S to be a 4. For each grade, multiply the 5 - 1 numerical equivalent of the grade times the number of credits (3 credits = major subject areas; 1 credit = minor subject areas). Divide the total of all the grades by the total number of credits to determine the GPA. For example:

history	B	(3 cr)	x 4 =	12
math	C	(3 cr)	x 3 =	9
science	C	(3 cr)	x 3 =	9
English	B	(3 cr)	x 4 =	12
shop	A	(1 cr)	x 5 =	5
phys ed	A	(1 cr)	x 5 =	5
music	C	(1 cr)	x 3 =	3
art	B	(1 cr)	x 4 =	4
		16 cr		59

$\frac{59}{16}$ = 3.87 GPA or 4*

*Note. It is necessary to round this number to a single digit number. In this instance, the 3.87 was rounded and would be coded as 4. For example, 3.50 through 4.49 should be rounded to 4, 2.50 through 3.49 should be rounded to 3, etc.

- In an ungraded section or primary grades, sometimes only the teacher comments are in the school records. Try to translate the key words in the report card to make a judgment about an overall superior, above average, average, below average, or poor/failing student. If the teacher writes only the difficult to code comments like "tries hard" or "works to capacity" as an overall evaluation of the student's performance, try to get the sense or overall tone of the report and classify using the coding system above. There may be an overall tone of "this is a satisfactory, or superior, or poor student." There will be several students in the same school district, so that some ranking may be possible with a principal's help. If the student's progress is evaluated with the words poor, fair, average, satisfactory, or superior for a particular level of reader in the primary unit, this can be translated arbitrarily into the above scheme.

Instructions for School Data on Underachievers-Achievers - Part B

The Consortium sample provides a rare opportunity to explore developmental issues, particularly the linkages between early education experiences and later outcomes. The Part B School Data Form is designed to collect developmental school or family histories on the students with school problems -- the underachievers (i.e., students placed in special education classes, and/or retained-in-grade, and/or dropped out of school). If a project is doing a comparative study, Part B can also be used for achievers.

The Part B School Form is a fishing expedition which attempts to cope with two problems: (a) to compress extensive case study material found in some school systems into a finite number of analyzable categories, or (b) to search out usable data from sparse or incomplete school folders.

ACYF has a strong interest in explanatory studies which focus on why programs worked and failed, and on developmental/ecological factors. The idea is to use the school records as raw data and search for clues to understand the school careers of the participants. The data will not be perfect but may provide some rich material from which to formulate research questions. Remember, it is only exploratory, so use whatever data you have access to. Some project sites will be able to obtain more information than others. We will use what we have.

For Part B, we have borrowed heavily from: Turnbull, A.P., Strickland, B.B., & Brantley, J.D. Developing and Implementing Individualized Education Programs (IEP). Columbus, Ohio: Charles C. Merrill Co., 1978. An IEP is an individualized education program which is now required for any students in need of special education or related services. The IEP requirement comes from the Education for All Handicapped Children Act which was passed in 1975. A good summary of the Act and its implications is: Abeson, A. & Zettel, J. The end of the quiet revolution: The Education for All Handicapped Children Act of 1975. Exception Children, October, 1977, pp. 114-128.

Again, to eliminate bias, the person who does the recording of the information from the folders should be blind to the research questions, if possible. Write legibly, and remember to insert cardboard behind the copy each time so that writing does not transfer to subsequent sheets.

Note. Do not leave blank boxes, use 9, 99, 999 (1, 2 or 3 digit codes) for no information, not applicable, don't know (except for office codes). Record some response for each item, e.g., if there is no health problem, record "none", or if no parental interest shown, record "no interest shown".

Instructions for each item follow:

#1 Record the project ID number and child ID number. Record the child ID number at the top of each page.

#2 If there are several years of evaluations in the student's folder, use one Part B Form for a given year or grade level. It isn't necessary to complete a Part B Form for every year so we suggest this priority system. It is most important to understand the student's characteristics prior to the first special class assignment or grade retention, therefore, complete one Part B Form for at least the year immediately before the special education services were first indicated (e.g., 04 = 4th grade; Kindergarten = 50; ungraded = 21) or as close to that time as possible. Then sample at least one additional year, either in junior high or high school, if possible. This will give us a developmental picture with at least two or three data points. For each Part B Form used (i.e., each yearly evaluation), fill in the earliest date of referral (evaluation) in the given year and the grade level of the student at the time of the evaluation. For example: if evaluations were done in 1/71, 2/71, and 4/71 record 01, 71.

#3,4,5 Source(s) of comments. Within a given year, there may be several sources for the evaluation. You may find reports, for example, from a teacher, nurse, psychologist, etc. Rather than fill out one Part B Form for each person's report, indicate the source of the comments with an initial in parentheses next to the comment. Code: (T) = teacher; (C) = counselor; (M) = physician; (MP) = psychiatrist; (MN) = neurologist; (N) = nurse, (P) = psychologist; (S) = social worker; (L) = learning disabilities specialist; (F) = family or parent. If you find other sources, let us know so we can add them to the code. Record, for example: "poor coordination" (S). If both teacher and psychologist make same comments, record "difficulty with spatial relations" (T) (P). If psychologist's report says that the teacher reports that child has a difficulty with peers, record original source, e.g., "difficulty with peers" (T). If two psychologists write different reports, it is sufficient to record (P).

#3 If there is one main reason, indicate with an asterisk (*). Main reasons might include, e.g., academic progress is unsatisfactory, constant distraction to class, fights a lot. If the school record or evaluation doesn't specifically indicate a reason for referral but the recorder is able to infer the reason(s) for referral, record the inferred reason, and indicate as follows: "long history of disturbed behavior" (recorder inferral). If it is really impossible to record a main reason, record, for example, "no reason indicated." Do not leave lines blank.

#4 Record diagnostic classification if indicated. For example, "Slow developmental pattern"; "unsocialized aggressive reaction of childhood"; "learning disability"; "socially maladjusted"; "emotionally disturbed"; certified for special education, physically handicapped, or any other special education or pupil personnel services. Diagnostic classification may be very important to us so that if a diagnosis is not indicated, the recorder should try to ascertain one. Sometimes the diagnostic classification is not indicated in the evaluation year, but you may find a diagnosis elsewhere in the folder. The diagnosis may be indicated in either an earlier or a later evaluation. Perhaps the psychologist recommended that the child be evaluated for "learning disability" and later the diagnosis was confirmed (or not confirmed). You may record that information as follows: "learning disabled" (unconfirmed). Indicate the source if the child is subsequently diagnosed, e.g., "emotionally disturbed" (indicated in 1974 follow-up letter), or "possibility child might have perceptual problem[(P) evaluation in subsequent year].

#5 Use the enclosed "Student Characteristics Indicative of Potential Problems (Figure 1)" to categorize the comments from school folders. When any of the problems are mentioned in the school folder, record the characteristic under one of the 7 problem areas (#5a - #5g). Write legibly with as much detail as possible so that we will be able to code the characteristics under each category. Use verbatim comments. For example, record "insecure" (P), "asked frequently for when mother would return", "relaxed when she returned" (P). Do not summarize by recording "mama's baby". See Attachment I -- "EXAMPLE JOHNNY", "EXAMPLE JOE", and "CRITERIA FOR SEVERITY OF PROBLEM". If you can't place a comment in one of the 7 categories, record in #6.

#5a Health (medical, physical, organic) problems which are current should be recorded here. Current, ongoing, organic or physical problems which are "visible" at present, e.g., physical disabilities, cerebral palsy, epilepsy, or accidents or corrective surgery within past year should be recorded in #5a. Previous problems, e.g., meningitis, convulsions, prematurity, anoxia, etc. should be listed in #10 with approximate or actual age or date. Medications or treatments should be recorded in #10.

#5f Record three types of information here: actual test scores; interpretations of test scores (e.g., "reads at one grade below level (P)") and generalizations about academic problems (e.g., "poor reading ability". See Figure 1 for other examples. Test scores may indicate a wide range of deviance or maladjustment from mild to severe. Grade scores more than one year below age expectancy of grade placement, percentiles below 25, grades D or F, and stanines of 3 or less imply a significant deviation in learning. Indicate year of test (or grade level), test name, results and interpretation, e.g., see Interpretation of Test Results in "EXAMPLE JOE".

#5g School staff may use the following criteria to determine the severity of a problem. "Unacceptable behavior consists of behaviors the teacher cannot tolerate, such as hitting, verbal abuse, and threats. Feelings of anger or loss of control over the child are internal cues to the teacher of the presence of this kind of behavior. Behaviors may be qualitatively unacceptable: hitting or biting or extremely frequent inappropriate behaviors such as wandering or leaving the class. These behaviors may signify substantial interference with the child's own learning or interfere with the learning or others. Behaviors which substantially interfere with the teacher's operation of class are of equal concern. Attention-getting behaviors of many kinds, individual needs for assistance with toileting or dressing and needs for special instructional strategies or materials that are difficult to accomodate in a large class are examples." (Source: Procedural Guidelines for IEP Development)

#6 Other comments that do not fit in categories in #5 can be recorded here.

#7 Indicate whether there is any evidence of parental interest and cooperation (interested, concerned, cooperative, etc.) Otherwise, record "no parental interest indicated." Record family problems in #10.

#8 Some school folders may indicate teacher's, psychologist's or someone else's judgments that will enable you to fill in #8a, #8b, and #8c. Otherwise, record 9s.

#9 This item concerns the school reports for only the given year. It is designed to provide detail that is not covered in Part A. Services received in subsequent years should be recorded in #10. Some districts may provide this information so that it can be transcribed into #9; for other projects, it may be impossible. Therefore, use judgment about how much effort should be devoted to collecting #9a, #9b, and #9c.

Be sure that Part B #9a, #9b, #9c, and #9d agree with Part A #5 and #5a. That is, if Part A #5a indicates learning disabled class replaces regular class time totally, record 30 hours in #9c and 000% in #9d.

#9a In the first column, the types of special services which may be offered in your district are listed. This continuum of services is taken from the IEP Sourcebook mentioned above. The services offered in your school district may differ. Let us know. Residential home, for example, should be specified under #9a "other". The "special class" category includes: special educ. unspecified, EMR, TMR, ED (including beh. disorder), learning disabled, perceptually handicapped.

#9b Record as many services as necessary. For example, if classroom teacher provided a special enrichment program (e.g., "Project Promise") record in both categories (regular class = 2 and resource teacher in regular classroom = 2).

For a given year, indicate whether the service was:
1 = recommended only, but not received (or no information whether received or not) in that year
2 = recommended and received in that year
3 = received, but no indication of recommendation in folder
4 = recommended and subsequently received (in a year other than the evaluation year) (Note: this code was added subsequently and is not on form.)

#9c If hours per week are indicated in a straightforward manner, we will have an indication of the extent and intensity of services actually received for that year. School districts place varying amounts of emphasis on special help based on financial constraints and interest. Record hours per week for each service based on 30 hours per week (9 AM - 3 PM). If a service was recommended only in that year, record 99.

#9d Record percentage of time in regular classroom in the information is given in a straightforward manner. If student recieved a supplemental service in an unknown amount, record 101.

#10 This space can be used to describe any other past events (positive or negative) or "treatments". The support systems can include, e.g., community agencies or services, medical or hospital treatments. For example, if a child is on Ritalin (a medication used for hyperactivity) record that information in #10 and record "present time" under date. Record previous medical (physical, organic) problems which may explain current problems (e.g., prematurity, anoxia, complications from childhood diseases, convulsions, fevers not within the present year) on the lines in #10. Use additional sheets (or back of page 3) to record additional events and supports. If you use back of page 3 to record comments, be careful to insert cardboard.

Figure 1: Student Characteristics Indicative of Potential Problems*

a. Health (medical, physical, organic) Note: list ongoing handicaps -- within the past year, here. Record previous health problems in #10.

Birth defects
Trauma, accidents within the past year
Allergies, asthma
Sleepiness, listlessness, fatigue
Frequent absence due to illness
Coughing, respiratory difficulty
Complains or suffers frequently
Dental problems
Obvious overweight or underweight
Bruises or injuries which appear repeatedly

b. Hearing

Frequent requests to repeat statements
Inability to differentiate sounds
Inattentiveness, daydreaming (if thought to be a hearing problem)
Close concentration on speaker's mouth
Speech problems which appear related to hearing difficulties
Comprehension of orally presented material
Listening comprehension/speech

c. Vision

Poor neuromuscular eye control
Physical irregularities -- eyes crossed, wavering, drooping lids, redness
Closeness to or unusual distance from work, squinting
Unable to distinguish colors, shapes, numbers, letters (matching, copying)
Head tilted or turned to one side when concentrating
Visual perception of word elements
Visual organization problems

---

* Partial source: 1978, Bell & Howell Co.

d. Speech

- Stuttering
- Articulation errors (distortions, substitutions)
- Lack of voice control (volume, pitch, quality)
- Difficulty in oral reading but good comprehension
- Pronunciation difficulties not due to language or dialect influence
- Expressive language
- Speech (intelligibility)
- Speech (erratic)

e. Perceptual-Motor Performance

- Large motor coordination: physical awkwardness, can't ride a bike, can't skate, etc., motor impersistence, balance problems
- Reversal of letters
- Poor fine-motor coordination (cutting with scissors, holding pencil, or writing)
- Poor visual or auditory memory
- Poor spatial relations
- Dyslexia
- Neurological impairments

f. Academic Achievement and Intelligence

- Achievement level one grade or more below expectancy in reading, math, or other area
- Needs many repetitions to learn
- Forgetful, poor memory
- Difficulty with abstract tasks
- Does not generalize principles or information from one task to another
- No systematic approach to work, trial-and-error style
- Poor reading ability
- Slow rate of learning or mastery
- Poor use of phonic skills, structural analysis (syllables, root words, etc.), context clues
- Problems with sight vocabulary, word recognition

Problems with getting main idea, singling out details, sequencing events, drawing inferences

Poor ability in following written directions

Poor recall of material

Word analysis problems

Reading comprehension problems

Spelling problems

Problems with Computation Skills

Meaning of bigger (more), smaller (less), same as (number vocabulary), place values (ones, tens, hundreds)

Counting orally

Counting objects

Sequencing alternate numbers (by 2's, 5's, 100's, etc.)

Meaning of +, -, = signs

Simple addition or subtraction facts

Regrouping (borrowing and carrying)

Multiplication tables, division tables

Telling time

Fractions

Decimals

Word problems

Money values

g. Social-Emotional Behavior

Short attention span, poor concentration

Anxiety, worry, nervousness, irritability

Impulsiveness, lack of impulse control

Poor emotional control/extreme emotions (anger, tantrums, silliness, fears, mood swings, sadness)

Habits (thumb-sucking, nail-biting, and hair-pulling)

Withdrawal, avoidance, social isolation, few friends

Relates poorly with peers

Relates poorly with adults

Refusal to do as asked

Lack of tolerance for tasks not enjoyed

Cannot wait or take turns

Leaves group or class (walks out, "sick")
Does not work with group
Overconforms to rules
Seeks attention excessively, needs constant attention
Hyperactivity, continuous talking, overly active, won't sit down
Cannot work alone
Easily confused or disoriented
Forgetful, needs constant reminders
Overly sensitive to criticism
Puts blame for behavior on external causes
Will not speak up
Shows off (clowning, bragging, teasing, etc.)
Speaks inappropriately (threatens, curses, etc.)
Antisocial tendencies (steals, lies, fights, etc.)
Hurries through work
Works too slowly
Easily distracted from work; distractability
Difficulty changing activities
Tires easily
Bothers peers
Doesn't do work
Violence toward peers
Cries easily
Mama's baby
Calls out in class

AN EXAMPLE OF IDENTIFICATION

Characteristics of Johnny Johnny is a 9-year-old white male who is achieving below grade level in every subject. Johnny came to the teacher's attention in the first few weeks of school when he had difficulty with work; however, the teacher's formal review of students at the end of the first grading period highlighted concerns about him. Johnny transferred to this school this year and was screened by the school nurse for health, vision, and hearing. He wears glasses, and an ophthalmologist's report indicates 20/40 vision in the left eye after correction; 20/20 right eye. Hearing is within normal limits. He is thin and restless. Johnny's parents are migrant laborers, and his mother is an American Indian who speaks an Indian language.

His teacher administered a screening test and found him to be 2 years below current grade placement in reading and arithmetic. Johnny's grades were all D's reflecting some effort but essentially no progress. On the playground, he is a reluctant participant in sports, has poor coordination, and is ignored by the other children outdoors and in the classroom. Johnny's teacher has tried both specific assignments with time limits and unstructured assignments without success. Johnny seems to respond better to instructions and work given to him verbally than when reading is required, and he works best in groups. He also prefers to speak, as his handwriting is very poor. Although he is cooperative and pays attention most of the time, he does not seem at all interested in school.

Analysis of Concerns The sources of concern identified in steps 1 and 2 are the teacher and school nurse, both of whom are inside the school. Teacher's observations of "difficulty with work," "poor coordination," and "handwriting is very poor" are informal kinds of information (step 3). Results of screening and grades are different kinds of formal information from the same source (step 4). Given this information, the teacher proceeds to categorize concerns in step 5, using Figure 4–2 as a reference. The following concerns were identified:

Characteristics	*Concerns*
Indian language, migrant status	Acculturation and Language
Thin, restless	Health
Vision: 20/20, 20/40 corrected	Vision
Poor coordination: poor handwriting, avoids sports	Perceptual-Motor Performance
Achievement: Two grades below expectancy, and grades of D	Academic Achievement and Intelligence
Relations with peers, avoids sports	Social-Emotional Behavior

In the categorizing of concerns (step 5), hearing and speech were eliminated from further consideration on the basis of hearing screening results and teacher observation. The migrant status of Johnny's family and the Indian language spoken by Johnny's mother raise concerns about acculturation and dominant language in the home. Nurse's observation that Johnny is thin and restless raises concerns about his health and nutrition. Corrected vision of 20/40, according to the nurse, is a concern, and Johnny's poor coordination and handwriting raise concerns about his perceptual-motor functioning. Achievement two grades below expectancy and grades of D raise concerns about academic achievement and intelligence. Social avoidance poses a concern with social-emotional behavior. Finally, Johnny's avoidance of sports may be related to social concerns and/or perceptual-motor difficulties, as illustrated above.

A home visit to estimate cultural diversity and language dominance testing were considered in step 6 as ways of determining whether acculturation and language are indeed concerns. (These activities may be left to the multidisciplinary team if referral is anticipated.)

EXAMPLE JOHNNY (CONT.)

Name Johnny Beam

Problem Areas

Acculturation & Language Mother American Indian who speaks Indian language

Health (Medical) Thin, restless

Hearing Screened OK (10/1/77)

Vision Wears glasses, Ophthalmologist's Report: right eye 20/20, left eye 20/40 (9/20/77)

Speech No interfering speech problems

Perceptual-Motor Performance Poor coordination, reluctant sports participant, poor handwriting

Academic Achievement & Intelligence Two grades below in reading and arithmetic, grades of D. No improvement with structure, time limits, different work levels. Verbal learner.

Social-Emotional Behavior Cooperative, attentive, but not interested in school

Parental Involvement None

Other Comments Teacher concerns, prior screening results, and grades are available on cumulative record. The principal has observed Johnny and verified concerns. Johnny's response to modifications in instruction has been minimal.

EXAMPLE JOE

The case utilized here was modified from the guide to IEP writing published by the North Carolina Department of Public Instruction.

Psychological and Educational Evaluation

Subject: Joe S.
Date of Birth: November, 1964
Date of Evaluation: September 12–14, 1976
Examiner: R.P. Jones, Ph.D., School Psychologist
Grade Level: Fifth

Referral: Joe was referred for evaluation largely because of poor academic progress. His teacher also commented that Joe seems to be a bit delayed in developing social skills.

According to available records, Joe began having academic difficulty in school in second grade. This difficulty, again according to Joe's teacher, increased when Joe was retained after third grade. To date, Joe is barely reading at third-grade level. He has some trouble participating appropriately in fifth-grade activities. He seems to lag behind in most areas. Joe's teacher says that his lack of participation has caused him to drop in popularity with the other children. He is somewhat isolated at times.

Behavioral Observations: Joe is a tall, blond, right-handed male with brown eyes and an engaging smile. He related quite well to the examiner although he appeared to be rather nervous. Joe's first comment, "You're going to give me some tests to see if I'm stupid, right?" revealed a real lack of confidence on his part. Despite assurances that this was not the case, Joe continued to make disparaging comments during the evaluation. As the testing began, Joe became very interested and involved in the tests.

Tests Administered:

Wechsler Intelligence Scale for Children—Revised
  Verbal IQ = 69
  Performance IQ = 68
  Full Scale IQ = 69

Peabody Individual Achievement Test
  General Information = 4.0

Woodcock Reading Mastery Tests
  Form A Letter Identification = 3.0
    Word Identification = 1.8
    Word Attack = 2.0
    Word Comprehension = 4.3

Informal (Examiner-made) Arithmetic Inventory
  Joe can compute up to the 100's place in addition and subtraction with carrying, regrouping and with zeroes.
  Joe can successfully compute the 100 basic multiplication facts.

Wide Range Achievement Test, Level II
  Reading Grade Level = 2.3
  Spelling Grade Level = 2.0
  Math Grade Level = 2.7

Interpretation of Test Results: Joe was functioning in the mildly retarded range of intelligence on the WISC-R, where he displayed approximately equal verbal and performance capabilities. There was, however, considerable variability in Joe's functioning across various subtests, with his poorest development in verbal comprehension. His most advanced performance was on a subtest requiring expressive vocabulary. There was also considerable intratest variability in Joe's functioning on the WISC-R. At times, he failed early items and then passed more difficult items on the same subtest. This variability seemed to relate to Joe's nervousness as well as to possible gaps in Joe's information and understanding.

Joe obtained a moderate score on the general information portion of the Peabody Individual Achievement Test. There was much scatter in his results, with answers falling as low as the second-grade and reaching as high as the sixth-grade level.

Joe's academic attainments, as measured by the Wide Range Achievement Test, were mid-second grade. Joe demonstrated poor word attack skills and considerable confusion with long and short vowels and two- and three-letter blends. Joe seems to have developed a barely functional sight vocabulary beyond which reading is practically impossible.

According to the Woodcock, Joe has poor word identification and word attack skills but surprisingly good word comprehension. He has difficulty identifying letters written in cursive writing, thus lowering his word identification performance. Again, there was a wide scatter of success on this test. Some successes indicated ability on a fifth-grade level.

Joe has very immature handwriting skills. He was asked to copy a model sentence from a second- and third-grade paragraph. He slowly copied one sentence and appeared to lose his place frequently. His handwriting is characterized by poor letter formation and poor word spacing as well as inappropriate awareness of punctuation. Joe's pencil grasp was awkward.

Recommendations. It is clear to this examiner that Joe is mildly retarded and could benefit from special class placement. He needs some individual help for his academic problem areas as well as some individual attention to build up his confidence.

The need for special class placement in a resource room for EMR students is reinforced by the fact that, over the years, Joe's IQ score and academic levels have decreased. It is important that Joe receive help as soon as possible to prevent any further slippage. Joe, in the examination, responded well to praise and encouragement. Given a setting more appropriate to his needs than the present one, Joe stands a good chance of progressing according to his abilities.

Completed IEP on Joe S.

What follows is an IEP form completed for Joe S., the child whom you have been following throughout this packet. This IEP has been filled out in many areas (math, reading, social competence, physical education, and science) in order to provide an example covering various subjects that might be included in an IEP. The IEPs you will write may or may not cover the same areas.

1979 SCHOOL DATA FORM-PART A

NOTE: Use 9 or 99 for no information or not applicable. Project ID ☐☐ Child ID ☐☐☐☐ File 7 Card 1 1-8

1. Normal Progression	2. Year	3. Subject Grade Level	4. Promotion	5. Special Class?	5a. Replace or Suplem't.	6. Other Special Progs.	7. Track	8. No. of Days Absent	
K	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	9-18
1	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	19-28
2	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	29-38
3	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	39-48
4	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	49-58
5	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	59-68
6	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	69-78
							* * * * *	* * 2	1-8
7	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	9-18
8	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	19-28
9	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	29-38
10	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	39-48
11	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	49-58
12	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	59-68
	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	69-78
							* * * * *	* * 3	1-8
	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	9-18
	19__ - __	☐☐	☐	☐☐ ____	☐	☐ ____	☐	☐☐	19-28
		50 kgtn 21 ungraded 20 incompl year 22 drop	1 reg pro 2 soc pro 3 retained 4 ungraded	01 reg 02 sp ed (unsp) 03 EMR 04 TMR 05 ED 06 LD 07 PH 08 gifted 09 other, specify 10 S/H	1 repl 2 supp. 3 inst. Describe in #23	1 no 2 yes, specify on line	1 slow 2 med/avg 3 high 4 voc/tech 5 reg/gen 6 coll prep	99 no data 97 if absent 97 days or more	

CHILD ID__________

8a. Reasons for extended absences (more than two weeks at time or over 50 days)____

____

Office Use
☐☐ 29-30
☐☐ 31-3

9. Junior High/Senior High Class Rank

Grade (e.g., 07=7th)	Class Rank	Class Size	
☐☐	☐☐☐	☐☐☐	33-4
☐☐	☐☐☐	☐☐☐	41-4
☐☐	☐☐☐	☐☐☐	49-5
☐☐	☐☐☐	☐☐☐	57-6
		9 9	65-6

10. Ever retained-in-grade?

1 No 2 Yes ☐ 67

a. Number of grades repeated ☐ 68

b. Grades repeated (04 = 4th grade, 50 = kindergarten) ☐☐ ☐☐ ☐☐ 69-74

11. Comments on progression (e.g., graduated early, dropped out or repeated but enough credit to graduate)

____

____

Office Use
☐☐ 75-76

12. Graduated from High School?

9 Not applicable 1 No 2 Yes, regular degree ☐ 77

3 High school equivalency degree (if indicated on record)

13a. If student is in school, specify current grade level (e.g., 09 = 9th grade; 13 = one year past high school) ☐☐ 78-79

20 = taking classes for H.S. equivalency 21 = ungraded
99 = not in school

b. If in post-high school program, specify____________________ ☐ 80

CHILD ID ________________

*	*	*	*	*	*	*	4	1-8

14. High School dropout?

1 No 2 Yes ☐ 9

a. If yes, record date of first dropout (e.g., Feb., 1972 = 02, 72) ☐☐ month ☐☐ year 10-13

Office Use

b. Reasons or comments (e.g., no explanation noted, excessive absences, pregnancies) ________________ ☐☐ 14-15

____

c. Re-entry date ☐☐ month ☐☐ year ☐☐ 16-21

Comments ________________________________

d. Subsequent dropout? ☐☐ month ☐☐ year ☐☐ 22-27

Comments ________________________________

e. What was the last grade level which student completed and received credit for? (e.g., 09 = 9th grade) ☐☐ 28-29

15. Suspension(s)?

1 No 2 Yes If yes, give date(s) (e.g., Feb., 1972 = 02, 72) and comments for each suspension ☐ 30

Office Use

a. 1st suspension ________________ ☐☐ ☐☐ ☐☐ 31-36

b. 2nd suspension ________________ ☐☐ ☐☐ ☐☐ 37-42

c. 3rd suspension ________________ ☐☐ ☐☐ ☐☐ 43-48

month year

16. Expulsion(s)?

1 No 2 Yes If yes, give date(s) and comments for each expulsion ☐ 49

Office Use

a. 1st expulsion ________________ ☐☐ ☐☐ ☐☐ 50-55

b. 2nd expulsion ________________ ☐☐ ☐☐ ☐☐ 56-61

month year

17. Honors, Awards, Offices Held (specify, e.g., Honor Society)

____ ☐☐ 62-63

____

CHILD ID ____________

* * * * * * * 4 1-8

14. High School dropout?

1 No 2 Yes ☐ 9

a. If yes, record date of first dropout (e.g., Feb., 1972 = 02, 72) ☐☐ ☐☐ 10-13
month year

Office Use

b. Reasons or comments (e.g., no explanation noted, excessive absences, pregnancies) ____________________ ☐☐ 14-15

____

c. Re-entry date ☐☐ ☐☐ ☐☐ 16-21
month year

Comments ____

d. Subsequent dropout? ☐☐ ☐☐ ☐☐ 22-27

Comments ____
month year

e. What was the last grade level which student completed and received credit for? (e.g., 09 = 9th grade) ☐☐ 28-29

15. Suspension(s)?

1 No 2 Yes If yes, give date(s) (e.g., Feb., 1972 = 02, 72) and comments for each suspension ☐ 30

Office Use

a. 1st suspension ____________________ ☐☐ ☐☐ ☐☐ 31-36

b. 2nd suspension ____________________ ☐☐ ☐☐ ☐☐ 37-42

c. 3rd suspension ____________________ ☐☐ ☐☐ ☐☐ 43-48
month year

16. Expulsion(s)?

1 No 2 Yes If yes, give date(s) and comments for each expulsion ☐ 49

Office Use

a. 1st expulsion ____________________ ☐☐ ☐☐ ☐☐ 50-55

b. 2nd expulsion ____________________ ☐☐ ☐☐ ☐☐ 56-61
month year

17. Honors, Awards, Offices Held (specify, e.g., Honor Society)

____ ☐☐ 62-63

____

CHILD ID ____________

18. Referral for psychological evaluation or individualized education program (IEP)? ☐ 64

1 No 2 Yes

Office Use ☐☐ 65-66

Comments ____

____

19a. Total number of schools attended? ☐☐ 67-68

b. Does this total include all schools attended since (and including) kindergarten?

1 No (e.g., no records before 5th grade) ☐ 69
Specify ____

2 Yes

20. College Entry Examination taken?

1 No 2 Yes 9 No information, not applicable ☐ 70

Test Name and Date: ____

21. Transcripts, letters of reference sent to other educational programs, schools or employers?

1 No 2 Yes 9 No information, not applicable ☐ 71

Describe request(s), whom and date(s)

Office Use ☐☐ 72-73

____

22. Basic Competency scores indicated?

1 No 2 Yes, scores recorded and enclosed on Achieve. Test Form ☐ 74

3 Yes, but scores not recorded on Achieve. Test Form. Indicate grade level (e.g., 9th) and test name ____________________

23. Other information in school records (e.g., probation, juvenile court, psychiatric care, correctional or psychiatric institution) Specify and record date(s) ____________________

Office Use ☐☐ 75-76

____

____

CHILD ID ____________

*	*	*	*	*	*	*	5	1-8

24. Optional: Course Marks (use code below)--fill in for selected grades. In each column, a - f must be reported for the same grade.

Codes for Course Marks (24abc below)

5 A (excellent, superior, outstanding)
4 B (above average or good)
3 C (average)
2 D (below average)
1 F, E, or U (failing, poor, or unsatisfactory)

7 S (satisfactory or passing)
9 no information

	Grade Level:	☐☐th	☐☐th	☐☐th	☐☐th	☐☐th	9-18
a.	Reading	☐	☐	☐	☐	☐	19-23
b.	Math	☐	☐	☐	☐	☐	24-28
c.	Science	☐	☐	☐	☐	☐	29-33
d.	No. of failing marks/yr	☐☐	☐☐	☐☐	☐☐	☐☐	34-43
e.	No. of courses/yr	☐☐	☐☐	☐☐	☐☐	☐☐	44-53
f.	GPA for year or teacher overall impression (use code below)	☐	☐	☐	☐	☐	54-58

Codes for GPA (24f above)

5 Excellent; mostly A's; in 90's; teacher evaluation mostly superior
4 Above average; mostly B's; in 80's; teacher comments mostly good
3 Average; mostly C's; in 70's; comments mostly average
2 Below average; mostly D's
1 F's, U's; comments poor

7 Satisfactory
8 Too variable to code in above
9 No information

Recorder's name ____________________

Date information was recorded ____________________
(Mo./Day/Yr.)

1/8/79

PART B - 1979 School Information-Problem Areas

1. Project ID ☐☐ Child ID ☐☐☐☐ File [2] Card [1] 1-8

2. Date of Referral (or Evaluation) ☐☐ mo. ☐☐ yr. Grade Level (eg 4th = 04) ☐☐ 9-14

[Record comments from folder into categories below. Use student characteristics list Figure 1 . Code source of comments: (T)=teacher; (C)=counselor; (N)=nurse; (P)=psychologist; (S)=social worker; (M)=physician; (F)=family,parent

Office Use

3. Main reason(s) for referral (evaluation): ______ ☐☐ 15-16
(Indicate main reason with *, if possible.)
______ ☐☐ 17-18
______ ☐☐ 19-20

4. Diagnostic classification (if indicated; e.g., perceptually impaired, slow developmental pattern).
______ ☐☐ 21-22

5. Broad problem areas - use Student Characteristics List

a. Health (Medical) ______ ☐☐ 23-24
______ ☐☐ 25-26

b. Hearing ______ ☐☐ 27-28
______

c. Vision ______ ☐☐ 29-30
______

d. Speech ______ ☐☐ 31-32
______

e. Perceptual-Motor Performance ______ ☐☐ 33-34
______ ☐☐ 35-36
______ ☐☐ 37-38

f. Academic Achievement & Intelligence ______ ☐☐ 39-40
______ ☐☐ 41-42
______ ☐☐ 43-44

g. Social-Emotional Behavior ______ ☐☐ 45-46
______ ☐☐ 47-48
______ ☐☐ 49-50

CHILD ID ____________

6. Other comments ____

7. Parental interest (e.g., none, cooperative) ____________________

Office Use

☐☐ 51-52

☐☐ 53-54

☐☐ 55-56

☐☐ 57-58

8. Teacher ratings (if indicated)

a. Compared with other children in class, child's general level of achievement is:
1 = far above average
2 = above average
3 = average
4 = below average
5 = far below average

☐ 59

b. Compared with other children in class, child's ability is:
1 = far above average
2 = above average
3 = average
4 = below average
5 = far below average

☐ 60

c. How disruptive is cnild in classroom?
1 = almost impossible to teach with him(her) in room
2 = often disrupts class activities
3 = about the same as other children
4 = you would hardly know s/he was there

☐ 61

CHILD ID______________

9a. Continuum of Services	9b. Service recommended or rec'd? 1-recommended only 2=received & recommended 3=rec'd & no data on recommend.	9c. Hours/week:	
Regular Class	☐	☐☐	62-64
Resource Teacher in regular classroom	☐	☐☐	65-67
Resource Room	☐	☐☐	68-70
Reading Specialist	☐	☐☐	71-73
		*******2	1-8
Speech/Language Therapist	☐	☐☐	9-11
Counselor	☐	☐☐	12-14
Special Class	☐	☐☐	15-17
Transition Class	☐	☐☐	18-20
Other, specify______________	☐	☐☐	21-23
Other, specify______________	☐	☐☐	24-26

9d. Percent of Time in Regular Classroom (e.g., 100%, 000%) ☐☐☐ 27-29

10. Describe any critical home or school events or support systems (both positive and negative) that may influence children's development and school success. Critical events, support systems or services may include illness in family, fire, marriages, deaths, births, separations, financial hardship, midyear teacher changes (for better or worse), unsettled home life, moving, summer camp experiences, special tutoring, summer school, new positive significant other person(s), etc. This item has a high priority for ACYF.

Date (or age)	Describe event
__________	____
__________	____
__________	____
__________	____
	____

Office Use	
☐☐	30-31
☐☐	32-33
☐☐	34-35
☐☐	36-37

Recorder's name______________

Date information was recorded ______________
(Mo./Day/Yr.)

12/15/78

PART C -- 1979 School Data Form -- School Policies (suggested questions)

As you know, we are doing longitudinal research, following the lives of the children who were enrolled in our preschool program in ____________________ and the children in the control group. We have been especially interested in their school careers: did they progress satisfactorily? What track or level were they enrolled in? What was their attitude towards school? We are hoping that school records can help us answer some of these questions. For example, we look for evidence of repeating grades, assignment to special education classes, teacher's ratings, absences from school, suspensions and expulsions, etc. To really understand what happened, however, we need to know about the school policies here, both current policies and policies in the past. We would very much appreciate your help.

1a. What kinds of special education classes are available for elementary school children in the district and at present?

____

(b) At present are there state or district mandated guidelines for assignment to special education teachers, like IQ score cutoffs, etc.?

____

(c) What are the current criteria for assigning elementary school children to special education classes or special education teachers?

____

____

(d) What is the actual procedure for getting a child into a special education classroom or assigned to a special education teacher? (For example, who refers the child, does s/he see a psychologist, etc.)

____

(e) Do (did) individual (teachers) (prindipals) (psychologists) really use these rules or guidelines in actual practice? How much autonomy do they have?

____

(f) Do you know if these same criteria guidelines and procedures were used 5 years ago? (10 years ago?) (If not, what were they?) (In the years our subjects (were in) school in Philadelphia from 1964-1976 ....)
(started)

____

2a. What kinds of special education classes are available for junior and Senior- high school students in this district?

b. What are the current criteria (guidelines) for assigning junior high and senior high school students to special education classes or special education teachers?

c. What is the actual procedure for getting a child into a special education classroom or assigned to a special education teacher?

d. Do you know if these same criteria and procedures were used 5 years ago? (10 years ago?) (If not, what were they?)

Now I have some questions about grade retentions.

3a. Some schools have a policy that students can't be retained more than once before sixth grade. What are the district or local policies about retaining a child at the elementary school level?

(Probe: who decides that a child should repeat a grade?)

b. What are the reasons that elementary school children, generally, have to repeat grades?

c. What are the local guidelines policies for retaining a student in junior high or high school? (not giving him/her credit)

d. Do you know if the same policies about retention were used 5 years ago? (10 years ago?) (If not, what were they?)

____

____

4a. What are the school policies for suspending elementary school children?

1. What are the reasons? ____________________
2. Kinds of suspensions? ____________________
3. # of suspensions before expulsion ____________________

b. What are the policies for suspending students at the junior high and senior high school levels?

1. What are the reasons? ____________________
2. Kinds of suspensions? ____________________
3. # of suspensions before expulsion ____________________

c. Do you know if the same policies for suspending students were used 5 years ago? (10 years ago?) (If no, detail differences.)

____

____

5a. What are current school policies for expelling elementary school children?
1. What are the reasons? ____________________
2. How long before the child can return to school? ____________________
3. Any others? ____________________

b. What are the policies for expelling students at the junior high and senior high school levels?
1. What are the reasons? ____________________
2. How long before the student can return? ____________________
3. Any others? ____________________

c. Do you know if the same policies were used to expell students 5 years ago? (10 years ago?) (If no, detail differences.)

6 At what age can students legally drop out of school?

a. Why do you think students drop out of school?

b. Does the district have any kind of drop-out prevention program? (Describe briefly)

c. What must a student do in order to re-enter school after dropping out?

d. Are there any special programs or services for students who drop out of school before graduation?

7a. Does this district have special policies for pregnant students?

b. Are there any programs or classes especially for pregnant students?

OPTIONAL:

8a. Does this district administer basic competency tests?

(Probe for: name/kind of test, grades when given.)

b. Do students with low scores receive any kind of remedial work?

____

____

c. When did the district start giving these tests?

____

9. Do you know if classroom teachers would be aware that certain children attended experimental preschool programs?

____

10. You mentioned several special education programs earlier. (Interviewer: see question 1) Could you try to rank those programs in order according to whether the program is for students with least serious up to most serious problems.

(6-15-79)

INSTRUCTIONS FOR 1979 INTERVIEW

Standard Interviewing Procedures

Standard procedures are necessary in order to obtain valid responses from the respondents and to achieve as high a degree of reliability among the various studies involved in this project as possible.

Setting the atmosphere. First of all, you will want to establish a fairly relaxed relationship with the respondent (R) to make him/her feel as comfortable as possible. For face-to-face interviews, set the interview up in an informal way, e.g., seated on two sides of a table or seated next to each other on a couch. You should not sit close enough so that the respondent can read ahead of you on the interview or your recording of the responses. Don't sit behind a desk, which may make the respondent feel s/he's being interrogated. For telephone interviews, see enclosed special instructions.

You will want to make the respondent feel relaxed and responsive, but you should not try to ingratiate yourself so that the respondent will try to please you be giving only answers s/he thinks you want to hear. Smile, be pleasant, act (sound) interested, look at the respondent when s/he is answering questions or when you are asking them as much as possible. Nod, say things like "I see," "Fine," "Good." If respondent says things like "I don't know if I'm answering right," say "You're doing fine, these are exactly the types of things I want to hear about."

Payment. The interview will have been set up by letter or by phone so that the respondent will know that s/he will be paid. You should pay the respondent at the end of the interview, either by check or by cash with a receipt being obtained from the respondent.

General Instructions for Using Precoded Interviews

1. Instructions to interviewer are written in ALL CAPITAL LETTERS on the interview form. There are not to be read to the respondent. The questions to be read are written in small letters. Read all questions exactly as written. Do not improvise. Suggested probes are included in these instructions and on the interview. Important: do not read choices to respondent unless instructions say: [READ ALTERNATIVES].

2. You are responsible for completing all questions on the interview. Be sure that you thoroughly understand all the questions, probes, and skip patterns before you begin the first interview. Try the interview out on a friend to make sure you understand it.

3. All coding boxes except those marked "FOR OFFICE USE" must be filled in by you before you turn in the completed interview. Do not leave blank boxes! If you have left out a question you must call the respondent back and get the answer. Do not guess what the answer would be. Fill in 9 (one column code) or 99 (two column code) for not applicable and 8 or 98 if respondent says "I don't know" unless otherwise specified.

4. Record answers verbatim (exactly as stated) on open-ended questions. For example, when you say "Tell me something you've done that made you feel proud of yourself," the respondent may say "the teacher said I did good work." Record every word so the response can be coded. Do not abbreviate by writing "does good school work."

   Write legibly. If your answers are not legible, they cannot be coded. Check this after interview is over. If you use probes, insert probe marks (P)at the place in the verbatim answer at which you used the probe. Indicate several probes by (P) (P) (P) etc. When the response is very brief, or you feel you are not getting all information, ask things like "anything else?" "Anything at all," "Could you tell me a little more about that?" or pause and try "uhuh". For example: if R gives a numerical range for a question, probe for a definite number. So, if R says "16 or 17" for age of first part-time job interviewer should indicate Ⓟ and say something like "would you say 16 years old or would you say 17 years old, I need to write down a specific number."

5. This is a precoded interview. You record the code number of the response in boxes opposite the questions. The numbers next to the boxes are the numbers of the columns on computer cards. The computer cards will be keypunched directly from these interview forms, so that it is essential that the numbers are entered in the boxes in a legible manner. Do not write in the "OFFICE USE ONLY" box and do not write in the keypunch area as it will be confusing to the keypuncher. Use leading 0 for one digit numbers in two-box codes, e.g., 5 years old is recorded 05; six months of Head Start is 06; January is 01.

6. The interview is printed on carbonless paper so that the original can be sent to Cornell and the copy retained at the individual project for security. Use a ball point pen and press firmly. Insert the cardboard behind the yellow carbon page so that the response is not inadvertently imprinted on the subsequent pages. When the interview goes back to the project office, the interview will be taken apart and the white originals stapled together to be sent to Cornell. There are special instructions to insure anonymity for the Illegal Activities Section. The interview will be coded and keypunched at Cornell and a duplicate set of cards returned to the project.

7. Please do not add any boxes for any reason. Extra information can be written in alongside box in a margin.

8. Please send the first 5 interviews to Cornell so that we can find any inconsistencies.

General Guidelines

1. Your interviewing package will include: interviews; interviewer instructions, several supplemental household enumeration sheets; cardboards to use with the carbonless interview forms; and cards for multiple choice questions.

2. Before the appointment with the respondent, write the Child ID number on the line at the top left of EVERY page so that if the pages of the interview are misplaced in the separating and restapling process, the complete interview will be intact.

3. The interview begins with the introductory statement read from Card 1. Note the time you begin the interview. Record the time on page 29. If respondent sounds skeptical about interviewer's identity, say: "You can call my office (phone number) to double check on who I am." For telephone interview, say "If you want to double check on who I am, call me back at this phone number_____.

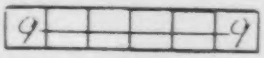
4. Insert the protective cardboard behind the yellow copy of page 2 in order to prevent your writing from going through the rest of the interview.

5. Record the Project ID, and the Child ID on top of page 2. Projects should assign their own interviewer ID numbers as they wish.

Instructions for Individual Questions

#1a,b,c First list the names of all members of the household living here now, more than half the time. Then go back and get the additional information on each person. (If the respondent does not want to give names they may use numbers to identify the people in the household.) Do not include respondent in listing. If there is difficulty determining if someone lives in the house, use the Decennial Census definition (SSRC, 1976): In general list everyone who usually lives in housing unit and persons staying or visiting who have no other home. List as a resident persons who live in household but are temporarily absent (on visit, vacation or on a job, in general or VA hospital); persons who stay away only on weekends; foreign citizens studying or working here. Do not list as residents persons who live in home on weekends only (because they work 5 days in another place); or if absent because live in long term institutions (jails, psychiatric, TB hospitals, nursing home, chronic ward, homes and schools for deaf, retarded); persons absent because live in monastery, military base, migratory camp, students (including nurses) who live away and come home only for vacations. If spouse died or separated during the year, s/he does not appear in listing. This is current household composition.

Be sure to fill in each space for each person. Record 9s in empty boxes. You may enter a 9 in left box and draw a horizontal (never vertical) line across to right hand box ending with a 9. For example,



#1c Record age as of last birthday (01 = 1 year old, 02 = 2 years, etc.). If child is less than one year old, record 00.

Note: Record total number in household in col. 69-70 (excluding respondent) regardless of whether there are more/less than 12 members. If more than 12 people in household, record total number in col. 69-70 and attach additional household sheets. Be sure to note that respondent is excluded from this household total as well as the household listing (#1a,b,c).

#1d The purpose of this detail is to tie information to school dropout return or job changes. If R has difficulty with exact date, say "do you remember if it was winter or summer?"

#2a R may say that at present they are living with someone in marriage-type relationship, and then volunteers the information that s/he has never been married. In this case, record 2 in #2a, i.e., current status = living with someone -- then ask #2b, do not ask #2c (alternate) -- simple record 0 in #2d and skip to #2e.

If R answers "no" to #2a and you learn from #2d or #2e that respondent has never been married, record 0 in #2a, column 25.

#3a If spouse/companion has post high school training, record number of years completed as 13, 14, etc. For example, for one year of nurses training or college or trade school, or 12 credits of night school, record 13; record 14 for 2 years post high school, etc.

#3d,e,f We want to obtain enough information to code jobs into NORC or Duncan pretige scores using Bureau of Census categories. The attached "Occupational Coding" will help you see if you have enough information. You may not need to ask #3d and #3f if R gives complete codeable response to #3d. Practice coding a few responses with the occupational coding scheme attached to see if you have sufficient information. (Attachments 1 & 2)

#3g If R gives you the name of a program, but you can't classify it, probe: "what is the purpose of (that program)?" If R says "college" ask "is that a 2-year college, a vocational school, a 4-year college, or what?" Use code 08 for an educational program in an institution or a residential center or a prison. Use code 09 for other educational programs that cannot be classified in 01 - 08.

Note: Columns 41-44. Armed services may not be an educational program in reality, but for our purposes it can be considered career development.

#5a If R says "night school," ask "Is it like a trade or vocational school, or a 2-year college or what?"

#9, 10a These questions may be answered in two ways. The R may say either I would like to go as far as I can go" or may give an amount. If R says "as far as I can go," it is important to determine how far R would like to go. Therefore, try to probe for the amount. Either repeat the question or ask "If you had your choice, how far would you like to go?" If R does not name a specific amount, record 10. If R gives a level of education after the probing, record 1 in col. 74 or 76 and then record (1-8) for the level hoped for in col. 75 or 77. Thus the answer "As far as I can go" (P) "I'd like to finish college" would be coded as 16.

#13c For example, 15=15 years old. If R gives a numerical range for a question, probe for a definite number. For example, if R says "16 or 17" for age of first PT job, interviewer should indicate (P) and say something like "would you say 16 years old or would you say 17 years old, I need to write down a specific number."

#13d For example, 05=5 jobs. If respondent has had no full-time jobs, record 00 in columns 42-43. If respondent has had no part-time jobs, record 00 in columns 44-45. If there is no information about full-time/part-time jobs record 99s.

#14d If R gives a number and a fraction for number of hours worked per week on last job, round up to the next whole number if the fraction is one-half or greater. For example, if R says "3 1/2" hours worked per week, report "4" hours worked per week.

#14e The only purpose of this question is to determine whether R can be classified as a wage earner or is a salaried worker. A person who is salaried is probably hired for at least the year and thus has more job security.

#14f If salary is reported on monthly or yearly basis either calculate "pay per hour" by using "hours worked per week" (#4d) or leave it blank and we will do it. If R says "minimum wage" for $/hour, again try to pin her/him down to a specific number.

#14i,j,k If R only names one item, probe "Anything else?" We are looking for codeable responses like (no) opportunity for advancement; (too much) supervision; (good/bad) coworkers, competition; (good/bad) pay; (dirty/clean) work; hours (too short/long). General comments like "good working conditions" are not codeable. You will have to probe, saying "tell me more about what you mean by that" or "can you give me an example?"

#14i,j,k Please probe the occupation adequately for a codeable response. For example, "office assistant" or "production" are not sufficient. Enclosed are copies of the Bureau of Census Occupational Classification. Interviewers should look this over to obtain the information we need. We may also code with the Hollingshead Scale. It has some quirks too. The Hollingshead Scale distinguishes between "waitress - better places" and "waitress - greasy spoon type", for example. (See attachments 1 & 2)

#15 If R has held several jobs, these questions should apply to the latest job. If not the latest job, specify which job the information refers to and why the latest job was not selected for this question.

#16b If more than one job seeking method is mentioned, record 6 in column 47, and then circle the code numbers of the methods that R mentions.

#18 If R is hesitant, you can repeat reassurances of confidentiality. If R still seems reluctant to discuss this topic, move to question #19.

#19a Read the five items with the code number. That is, say, "One, high income; two, no danger of being fired, etc." After R tells you which job characteristic is most important, ask "Which is next most important?" For telephone interview, you will have to read all remaining items with the code number each time.

#21 If R has already mentioned his/her own children in the household composition question, then ask about any other children.

#21b The child's sex is indicated because there may be sex differences in whether a mother decides to keep a child at home.

#22,23 If R gives a numerical range, probe for a definite number. For example, if R says "maybe one or two children", interviewer should indicate (P) and say something like "would you say one or would you say two children, I need to write a specific number."

#24 Some women have already told you they have children. Obviously if R has said she has children, she was pregnant, so question #24 serves to determine if she is pregnant now. Say, "you told me about the children you have already; are you pregnant now?" Use code "2" if not pregnant now; use code "4" if she says she's pregnant now.

#26 Give a copy of the #26 questions to R, but read introduction. Then read each item and record response 1, 2, 3 or 4. Repeat instructions as necessary. "Do you strongly agree, agree, disagree, or strongly disagree that the statement applies to you?"

#34 If R says "unemployment agency" you should probe to differentiate between #34a and #34c. In New York State in order to collect unemployment insurance, you must register with the State Employment Office (to indicate that you are looking for work) and then with the State Employment Insurance Office (to collect the money). If R says "unemployment agency," record the response in the margin and then you might ask "Do you mean where they try to find you a job or where they give you unemployment checks?" Then you can code yes for #34a and/or #34c.

ATTACHMENT 1

Alphabetical Index of Industries and Occupations, 1970
"Alpha Index" used by Bureau of Census

OCCUPATIONAL CLASSIFICATION SYSTEM

Equivalent alphabetic codes follow some codes. Either code may be utilized, depending on the processing method. "N.e means "not elsewhere classified."

Occupation Code	PROFESSIONAL, TECHNICAL, AND KINDRED WORKERS
001	Accountants
002	Architects
	Computer specialists
003	Computer programmers
004	Computer systems analysts
005	Computer specialists, n.e.c.
	Engineers
006	Aeronautical and astronautical engineers
010	Chemical engineers
011	Civil engineers
012	Electrical and electronic engineers
013	Industrial engineers
014	Mechanical engineers
015	Metallurgical and materials engineers
020	Mining engineers
021	Petroleum engineers
022	Sales engineers
023	Engineers, n.e.c.
024	Farm management advisors
025	Foresters and conservationists
026	Home management advisors
	Lawyers and judges
030	Judges
031	Lawyers
	Librarians, archivists, and curators
032	Librarians
033	Archivists and curators
	Mathematical specialists
034	Actuaries
035	Mathematicians
036	Statisticians
	Life and physical scientists
042	Agricultural scientists
043	Atmospheric and space scientists
044	Biological scientists
045	Chemists
051	Geologists
052	Marine scientists
053	Physicists and astronomers
054	Life and physical scientists, n.e.c.
055	Operations and systems researchers and analysts
056	Personnel and labor relations workers
	Physicians, dentists, and related practitioners
061	Chiropractors
062	Dentists
063	Optometrists
064	Pharmacists
065	Physicians, medical and osteopathic
071	Podiatrists
072	Veterinarians
073	Health practitioners, n.e.c.

Occupation Code	PROFESSIONAL, TECHNICAL, AND KINDRE WORKERS–Continued
	Nurses, dietitians, and therapists
074	Dietitians
075	Registered nurses
076	Therapists
	Health technologists and technicians
080	Clinical laboratory technologists and technician
081	Dental hygienists
082	Health record technologists and technicians
083	Radiologic technologists and technicians
084	Therapy assistants
085	Health technologists and technicians, n.e.c.
	Religious workers
086	Clergymen
090	Religious workers, n.e.c.
	Social scientists
091	Economists
092	Political scientists
093	Psychologists
094	Sociologists
095	Urban and regional planners
096	Social scientists, n.e.c.
	Social and recreation workers
100	Social workers
101	Recreation workers
	Teachers, college and university
102	Agriculture teachers
103	Atmospheric, earth, marine, and space teachers
104	Biology teachers
105	Chemistry teachers
110	Physics teachers
111	Engineering teachers
112	Mathematics teachers
113	Health specialties teachers
114	Psychology teachers
115	Business and commerce teachers
116	Economics teachers
120	History teachers
121	Sociology teachers
122	Social science teachers, n.e.c.
123	Art, drama, and music teachers
124	Coaches and physical education teachers
125	Education teachers
126	English teachers
130	Foreign language teachers
131	Home economics teachers
132	Law teachers
133	Theology teachers
134	Trade, industrial, and technical teachers
135	Miscellaneous teachers, college and university
140	Teachers, college and university, subject specified

Source: Featherman, D.L., Sobel, M., & Dickens, D. A Manual for Coding Occupations and Industries into Detailed 1970 Categories and a Listing of 1970-Basis Duncan Socio-economic and NORC Prestige Scores. Center for Demography and Ecology, Univ. of Wisconsin, Madison, Working Paper #75-1.

Code	WORKERS- Continued
	Teachers, except college and university
141	Adult education teachers
142 (N)	Elementary school teachers
143	Prekindergarten and kindergarten teachers
144	Secondary school teachers
145	Teachers, except college and university, n.e.c.
	Engineering and science technicians
150	Agriculture and biological technicians, except health
151	Chemical technicians
152	Draftsmen
153	Electrical and electronic engineering technicians
154	Industrial engineering technicians
155	Mechanical engineering technicians
156	Mathematical technicians
161	Surveyors
162	Engineering and science technicians, n.e.c.
	Technicians, except health, and engineering and science
163	Airplane pilots
164	Air traffic controllers
165	Embalmers
170	Flight engineers
171	Radio operators
172	Tool programmers, numerical control
173	Technicians, n.e.c.
174	Vocational and educational counselors
	Writers, artists, and entertainers
175	Actors
180	Athletes and kindred workers
181	Authors
182	Dancers
183	Designers
184	Editors and reporters
185	Musicians and composers
190	Painters and sculptors
191	Photographers
192	Public relations men and publicity writers
193	Radio and television announcers
194	Writers, artists, and entertainers, n.e.c.
195	Research workers, not specified

MANAGERS AND ADMINISTRATORS, EXCEPT FARM

201	Assessors, controllers, and treasurers; local public administration
202	Bank officers and financial managers
203	Buyers and shippers, farm products
205	Buyers, wholesale and retail trade
210	Credit men
211	Funeral directors
212	Health administrators
213	Construction inspectors, public administration
215	Inspectors, except construction, public administration
216	Managers and superintendents, building
220	Office managers, n.e.c.
221	Officers, pilots, and pursers; ship
222	Officials and administrators, public administration, n.e.c.
223	Officials of lodges, societies, and unions
224	Postmasters and mail superintendents
225	Purchasing agents and buyers, n.e.c.
226	Railroad conductors

Code	FARM - Continued
230	Restaurant, cafeteria, and bar managers
231	Sales managers and department heads, retail trade
233	Sales managers, except retail trade
235	School administrators, college
240	School administrators, elementary and secondary
245	Managers and administrators, n.e.c.

SALES WORKERS

260	Advertising agents and salesmen
261	Auctioneers
262	Demonstrators
264	Hucksters and peddlers
265	Insurance agents, brokers, and underwriters
266	Newsboys
270	Real estate agents and brokers
271	Stock and bond salesmen
280	Salesmen and sales clerks, n.e.c.[1]

CLERICAL AND KINDRED WORKERS

301	Bank tellers
303	Billing clerks
305 (P)	Bookkeepers
310	Cashiers
311	Clerical assistants, social welfare
312	Clerical supervisors, n.e.c.
313	Collectors, bill and account
314	Counter clerks, except food
315	Dispatchers and starters, vehicle
320	Enumerators and interviewers
321	Estimators and investigators, n.e.c.
323	Expediters and production controllers
325	File clerks
326	Insurance adjusters, examiners, and investigators
330	Library attendants and assistants
331	Mail carriers, post office
332	Mail handlers, except post office
333	Messengers and office boys
334	Meter readers, utilities
	Office machine operators
341	Bookkeeping and billing machine operators
342	Calculating machine operators
343	Computer and peripheral equipment operators
344	Duplicating machine operators

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[1] Category "280 Salesmen and sales clerks, n.e.c." was subdivided in the Census into 5 occupation groups dependent on industry. The industry codes are shown in parentheses.

Occ. Code	
281	Sales representatives, manufacturing industries (Ind. 107-399)
282	Sales representatives, wholesale trade (Ind. 017-058, 507-599)
283	Sales clerks, retail trade (Ind. 608-699 except 618, 639, 649, 667, 668, 688)
284	Salesmen, retail trade (Ind. 607, 618, 639, 649, 667, 668, 688)
285	Salesmen of services and construction (Ind. 067-078, 407-499, 707-947)

Occupation Code	CLERICAL AND KINDRED WORKERS--Continued
	Office machine operators Continued
345	Key punch operators
350	Tabulating machine operators
355	Office machine operators, n.e.c.
360	Payroll and timekeeping clerks
361	Postal clerks
362	Proofreaders
363	Real estate appraisers
364	Receptionists
	Secretaries
370	Secretaries, legal
371	Secretaries, medical
372 (Q)	Secretaries, n.e.c.
374	Shipping and receiving clerks
375	Statistical clerks
376	Stenographers
381	Stock clerks and storekeepers
382	Teacher aides, exc. school monitors
383	Telegraph messengers
384	Telegraph operators
385	Telephone operators
390	Ticket, station, and express agents
391	Typists
392	Weighers
394	Miscellaneous clerical workers
395	Not specified clerical workers

CRAFTSMEN AND KINDRED WORKERS

401	Automobile accessories installers
402	Bakers
403	Blacksmiths
404	Boilermakers
405	Bookbinders
410	Brickmasons and stonemasons
411	Brickmasons and stonemasons, apprentices
412	Bulldozer operators
413	Cabinetmakers
415 (R)	Carpenters
416	Carpenter apprentices
420	Carpet installers
421	Cement and concrete finishers
422	Compositors and typesetters
423	Printing trades apprentices, exc. pressmen
424	Cranemen, derrickmen, and hoistmen
425	Decorators and window dressers
426	Dental laboratory technicians
430	Electricians
431	Electrician apprentices
433	Electric power linemen and cablemen
434	Electrotypers and stereotypers
435	Engravers, exc. photoengravers
436	Excavating, grading, and road machine operators; exc. bulldozer
440	Floor layers, exc. tile setters
441	Foremen, n.e.c.
442	Forgemen and hammermen
443	Furniture and wood finishers
444	Furriers
445	Glaziers
446	Heat treaters, annealers, and temperers
450	Inspectors, scalers, and graders, log and lumber
452	Inspectors, n.e.c.

Occupation Code	CRAFTSMEN AND KINDRED WORKER Continued
453	Jewelers and watchmakers
454	Job and die setters, metal
455	Locomotive engineers
456	Locomotive firemen
461	Machinists
462	Machinist apprentices
	Mechanics and repairmen
470	Air conditioning, heating, and refrigeration
471	Aircraft
472	Automobile body repairmen
473 (S)	Automobile mechanics
474	Automobile mechanic apprentices
475	Data processing machine repairmen
480	Farm implement
481	Heavy equipment mechanics, incl. diesel
482	Household appliance and accessory installe mechanics
483	Loom fixers
484	Office machine
485	Radio and television
486	Railroad and car shop
491	Mechanic, exc. auto, apprentices
492	Miscellaneous mechanics and repairmen
495	Not specified mechanics and repairmen
501	Millers; grain, flour, and feed
502	Millwrights
503	Molders, metal
504	Molder apprentices
505	Motion picture projectionists
506	Opticians, and lens grinders and polishers
510	Painters, construction and maintenance
511	Painter apprentices
512	Paperhangers
514	Pattern and model makers, exc. paper
515	Photoengravers and lithographers
516	Piano and organ tuners and repairmen
520	Plasterers
521	Plasterer apprentices
522	Plumbers and pipe fitters
523	Plumber and pipe fitter apprentices
525	Power station operators
530	Pressmen and plate printers, printing
531	Pressman apprentices
533	Rollers and finishers, metal
534	Roofers and slaters
535	Sheetmetal workers and tinsmiths
536	Sheetmetal apprentices
540	Shipfitters
542	Shoe repairmen
543	Sign painters and letterers
545	Stationary engineers
546	Stone cutters and stone carvers
550	Structural metal craftsmen
551	Tailors
552	Telephone installers and repairmen
554	Telephone linemen and splicers
560	Tile setters
561	Tool and die makers
562	Tool and die maker apprentices
563	Upholsterers
571	Specified craft apprentices, n.e.c.
572	Not specified apprentices

Code	Continued
575	Craftsmen and kindred workers, n.e.c.
580	Former members of the Armed Forces

OPERATIVES, EXCEPT TRANSPORT

601	Asbestos and insulation workers
602 (T)	Assemblers
603	Blasters and powdermen
604	Bottling and canning operatives
605	Chainmen, rodmen, and axmen; surveying
610	Checkers, examiners, and inspectors; manufacturing
611	Clothing ironers and pressers
612	Cutting operatives, n.e.c.
613	Dressmakers and seamstresses, except factory
614	Drillers, earth
615	Dry wall installers and lathers
620	Dyers
621	Filers, polishers, sanders, and buffers
622	Furnacemen, smeltermen, and pourers
623	Garage workers and gas station attendants
624	Graders and sorters, manufacturing
625	Produce graders and packers, except factory and farm
626	Heaters, metal
630	Laundry and dry cleaning operatives, n.e.c.
631	Meat cutters and butchers, exc. manufacturing
633	Meat cutters and butchers, manufacturing
634	Meat wrappers, retail trade
635	Metal platers
636	Milliners
640	Mine operatives, n.e.c.
641	Mixing operatives
642	Oilers and greasers, exc. auto
643	Packers and wrappers, except meat and produce
644	Painters, manufactured articles
645	Photographic process workers
	Precision machine operatives
650	Drill press operatives
651	Grinding machine operatives
652	Lathe and milling machine operatives
653	Precision machine operatives, n.e.c.
656	Punch and stamping press operatives
660	Riveters and fasteners
661	Sailors and deckhands
662	Sawyers
663	Sewers and stitchers
664	Shoemaking machine operatives
665	Solderers
666	Stationary firemen
	Textile operatives
670	Carding, lapping, and combing operatives
671	Knitters, loopers, and toppers
672	Spinners, twisters, and winders
673	Weavers
674	Textile operatives, n.e.c.
680	Welders and flame cutters
681	Winding operatives, n.e.c.
690	Machine operatives, miscellaneous specified
692	Machine operatives, not specified
694	Miscellaneous operatives
695	Not specified operatives

Code	
701	Boatmen and canalmen
703	Bus drivers
704	Conductors and motormen, urban rail transit
705	Deliverymen and routemen
706	Fork lift and tow motor operatives
710	Motormen; mine, factory, logging camp, etc.
711	Parking attendants
712	Railroad brakemen
713	Railroad switchmen
714	Taxicab drivers and chauffeurs
715 (U)	Truck drivers

LABORERS, EXCEPT FARM

740	Animal caretakers, exc. farm
750	Carpenters' helpers
751 (V)	Construction laborers, exc. carpenters' helpers
752	Fishermen and oystermen
753	Freight and material handlers
754	Garbage collectors
755	Gardeners and groundskeepers, exc. farm
760	Longshoremen and stevedores
761	Lumbermen, raftsmen, and woodchoppers
762	Stock handlers
763	Teamsters
764	Vehicle washers and equipment cleaners
770	Warehousemen, n.e.c.
780	Miscellaneous laborers
785	Not specified laborers

FARMERS AND FARM MANAGERS

801 (W)	Farmers (owners and tenants)
802	Farm managers

FARM LABORERS AND FARM FOREMEN

821	Farm foremen
822	Farm laborers, wage workers
823	Farm laborers, unpaid family workers
824	Farm service laborers, self-employed

SERVICE WORKERS, EXC. PRIVATE HOUSEHOLD

	Cleaning service workers
901	Chambermaids and maids, except private household
902	Cleaners and charwomen
903 (X)	Janitors and sextons
	Food service workers
910	Bartenders
911	Busboys
912	Cooks, except private household
913	Dishwashers
914	Food counter and fountain workers
915 (Y)	Waiters
916	Food service workers, n.e.c., except private household

Occupation Code	SERVICE WORKERS, EXC. PRIVATE HOUSEHOLD—Continued
	Health service workers
921	Dental assistants
922	Health aides, exc. nursing
923	Health trainees
924	Lay midwives
925	Nursing aides, orderlies, and attendants
926	Practical nurses
	Personal service workers
931	Airline stewardesses
932	Attendants, recreation and amusement
933	Attendants, personal service, n.e.c.
934	Baggage porters and bellhops
935	Barbers
940	Boarding and lodging house keepers
941	Bootblacks
942	Child care workers, exc. private household
943	Elevator operators
944	Hairdressers and cosmetologists
945	Personal service apprentices
950	Housekeepers, exc. private household
952	School monitors
953	Ushers, recreation and amusement
954	Welfare service aides
	Protective service workers
960	Crossing guards and bridge tenders
961	Firemen, fire protection
962	Guards and watchmen
963	Marshals and constables
964	Policemen and detectives
965	Sheriffs and bailiffs

Occupation Code	PRIVATE HOUSEHOLD WORKERS
980	Child care workers, private household
981	Cooks, private household
982	Housekeepers, private household
983	Laundresses, private household
984 (Z)	Maids and servants, private household
995	OCCUPATION NOT REPORTED[2]
	ALLOCATION CATEGORIES[3]
196	Professional, technical, and kindred workers -allocat
246	Managers and administrators, except farm—allocated
296	Sales workers—allocated
396	Clerical and kindred workers—allocated
586	Craftsmen and kindred workers—allocated
696	Operatives, except transport—allocated
726	Transport equipment operatives -allocated
796	Laborers, except farm -allocated
806	Farmers and farm managers—allocated
846	Farm laborers and farm foremen—allocated
976	Service workers, exc. private household - allocated
986	Private household workers—allocated

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[2] This code is used to identify not reported occupations in surveys where the not reported cases are not allocated.

[3] Those returns from the Population Census which do not have an occupation entry are allocated among the major occupation groups during computer processing. These cases are labeled with the code for the "allocation" category to which they are assigned. (See text, page VI).

APPENDIX B

1970 Census Occupation Code	Duncan SEI Male Scores	Duncan SEI Total Scores	Siegel (1965 NORC) Prestige Male Scores	Siegel (1965 NORC) Prestige Total Scores
001	76.8	76.9	55.9	56.0
002	85.2	85.3	66.7	
003	65.0		50.6	
004	65.0		50.6	
005	65.0		50.6	
006	87.0		71.1	
010	89.9		67.2	
011	84.0		67.8	
012	84.0		69.4	
013	85.5		55.6	
014	80.2		62.1	
015	83.1		58.4	
020	85.0		61.6	
021	81.0		57.1	
022	87.0		50.6	
023	86.9		67.0	
024	83.0		53.9	
025	48.0		53.9	
026	83.0		53.9	
030	93.0		75.7	
031	92.3		75.1	
032	60.0		54.6	
033	74.6		59.6	
034	81.0		55.4	
035	80.0		65.0	
036	81.0		55.4	
042	80.0		55.8	
043	62.0		47.0	
044	80.0		67.7	
045	79.4		67.1	67.3
051	80.0		67.2	
053	80.0		73.8	
054	77.2	77.4	64.8	65.1
055	65.7		50.8	
056	83.6	82.4	55.8	55.2
061	75.0		60.0	
062	96.0		73.6	
063	79.0		62.0	
064	81.3	81.4	60.3	
065	92.1		81.2	
071	58.0		36.7	
072	78.0		59.7	
074	39.0		52.1	
075	44.3		60.1	
076	59.9	58.9	40.5	38.5
080	48.0		61.0	
081	48.0		61.0	
082	60.0		54.6	
083	48.0		61.0	
084	48.0		61.0	
085	52.2	55.2	49.8	51.1

1970 Census Occupation Code	Duncan SEI Male Scores	Duncan SEI Total Scores	Siegel (1965 NORC) Prestige Male Scores	Siegel (1965 NORC) Prestige Total Scores
086	52.0		69.0	
090	56.7	57.1	55.0	54.6
091	74.4	74.3	53.6	53.5
093	81.0		71.4	
095	65.0		50.6	
096	81.0		65.6	
100	64.0		52.4	
101	67.0		48.6	
102	84.0		78.3	
103	84.0		78.3	
104	84.0		78.3	
105	84.0		78.3	
110	84.0		78.3	
111	84.0		78.3	
112	84.0		78.3	
113	84.0		78.3	
114	84.0		78.3	
115	84.0		78.3	
116	84.0		78.3	
120	84.0		78.3	
121	84.0		78.3	
122	84.0		78.3	
123	53.2	55.6	46.8	48.6
124	64.0		53.2	
125	84.0		78.3	
126	84.0		78.3	78.3
130	84.0		78.3	
132	84.0		78.3	
133	84.0		78.3	
134	84.0		78.3	
135	84.0		78.3	
140	84.0		78.3	
141	61.3	64.3	44.3	43.9
142	71.2	71.4	58.9	59.2
143	72.0		56.1	
144	70.2	70.5	59.8	60.1
145	62.3	57.7	44.2	44.9
150	62.0		47.2	
151	62.0		47.0	
152	67.0		56.1	
153	62.0		51.6	51.5
154	64.1	64.0	49.5	49.4
155	62.0		47.0	
161	48.4		53.1	
162	62.0		47.0	
163	79.0		70.1	
164	69.0		42.8	
165	60.8	60.3	51.7	51.9
170	48.0		48.2	

1970 Census Occupation Code	Duncan SEI Male Scores	Duncan SEI Total Scores	Siegel (1965 NORC) Prestige Male Scores	Siegel (1965 NORC) Prestige Total Scores
171	69.0		42.8	
172	62.0		47.0	
173	62.0		47.2	
174	65.0		50.6	
175	60.0		55.0	
180	59.4	60.2	51.8	52.1
181	76.0		59.8	
182	45.0		37.6	
183	70.5	70.4	56.5	
184	82.0		51.2	
185	52.0		46.0	
190	67.0		56.2	
191	50.0		40.5	
192	82.0		56.7	
193	65.0		50.6	
194	40.2	45.4	38.6	41.2
195	65.0		50.6	
201	61.2	58.8	50.9	53.8
202	79.5	80.0	66.1	66.6
203	50.5	50.3	43.0	42.9
205	72.1		50.0	
210	74.0		48.8	
211	59.0		52.2	
212	74.1	56.9	63.8	59.4
213	57.6	57.5	39.6	
215	66.7	66.6	42.3	
216	32.0		38.3	
220	75.1	73.1	57.6	56.0
221	49.9		56.7	
222	67.3	66.5	60.7	60.3
223	59.8	60.0	48.4	48.4
224	61.3	60.8	58.4	58.3
225	74.7	74.8	46.4	
226	58.2		40.9	
230	37.6	38.1	38.7	38.9
231	70.6	69.8	48.5	47.8
233	74.7	74.6	54.2	
235	77.9	77.1	70.6	69.6
240	71.7		61.7	61.6
245	62.0	61.7	50.8	50.7
260	66.1		42.3	
261	40.0		31.9	
262	35.0	38.1	28.3	30.6
264	08.8	12.9	18.6	20.2
265	66.0		46.8	
266	27.0		15.4	
270	62.0		44.0	
271	72.3	72.4	51.2	51.3
281	65.0		49.1	
282	60.9		39.9	
283	39.0		28.7	28.6
284	39.0		28.6	
285	52.7	52.2	35.8	35.4

1970 Census Occupation Code	Duncan SEI Male Scores	Duncan SEI Total Scores	Siegel (1965 NORC) Prestige Male Scores	Siegel (1965 NORC) Prestige Total Scores
301	52.0	51.8	49.5	49.0
303	44.0		36.2	
305	50.8	50.9	47.3	47.4
310	44.0	43.9	31.4	31.0
312	43.6	43.8	35.8	36.0
313	43.3	42.5	28.4	27.9
314	44.0		36.2	
315	39.9	40.1	33.3	33.4
320	44.0		36.2	
321	59.2	56.2	42.9	41.2
323	43.7	43.6	36.0	
325	44.0		31.4	30.8
326	62.1		47.6	
330	44.0	43.1	40.4	39.7
331	53.0		42.3	
332	43.0	43.3	35.1	35.5
333	28.2		19.4	19.3
334	44.0		36.2	
341	44.9		43.7	44.3
342	45.0		44.9	
343	45.0		44.9	
344	45.0		44.9	
345	45.0		44.9	
350	45.0		44.9	
355	45.0		44.9	
360	44.0		41.2	41.3
361	44.7	44.6	42.3	42.4
362	44.0		36.2	
363	67.8		43.0	
364	44.0		37.1	38.9
370	61.0		45.8	
371	61.0		45.8	
372	61.9	61.0	46.5	45.8
374	24.2	24.1	29.9	
375	43.7	43.9	35.8	36.0
376	61.0		43.3	
381	44.0	43.9	25.0	25.2
382	63.2	62.4	49.3	48.8
383	22.0		29.8	
384	47.0		43.5	
385	45.0		40.4	
390	59.8		35.4	
391	61.0		41.3	
392	41.9	41.8	35.5	35.4
394	43.7		36.2	36.5
395	44.0		36.2	
401	21.6		32.5	
402	21.9	21.7	34.0	33.8
403	16.0		35.5	
404	32.6		30.6	
405	39.0	38.0	31.3	31.4

1970 Census Occupation Code	Duncan SEI Male Scores	Duncan SEI Total Scores	Siegel (1965 NORC) Prestige Male Scores	Siegel (1965 NORC) Prestige Total Scores
410	27.0		35.7	
411	32.0		40.8	
412	19.7		32.3	
413	22.3		38.1	
415	18.9		39.7	39.6
416	31.0		40.8	
420	12.0		32.8	
421	19.0		31.6	
422	52.0		38.0	
423	40.0		40.8	
424	21.0		38.7	
425	40.0		37.4	
426	48.0		61.0	
430	44.0		49.2	
431	37.0		40.8	
433	49.0		39.2	
434	55.0		38.0	
435	47.0		41.2	
436	22.8		31.5	
440	17.3	17.4	31.4	31.8
441	49.7	49.5	45.3	45.3
442	23.0	22.8	35.5	
443	17.8		29.1	
444	39.5	33.4	35.2	32.3
445	25.2	25.3	26.7	
446	21.7		35.3	
450	22.4	22.5	31.0	
452	41.2	41.0	31.3	
453	36.4		37.5	37.5
454	33.5		46.4	
455	57.8		50.8	
456	45.0		36.2	
461	32.9		47.7	
462	41.0		40.8	
470	27.0		36.7	
471	48.0		48.2	
472	19.0		36.7	
473	19.0		36.7	
474	25.0		40.8	
480	27.0		32.6	
481	26.6		32.8	
482	27.0		32.6	
483	10.0		30.4	
484	35.9		33.8	
485	36.0		35.0	
486	20.5		35.6	
491	34.0		40.8	
492	26.5		32.8	
495	27.0		32.6	

1970 Census Occupation Code	Duncan SEI		Siegel (1965 NORC) Prestige	
	Male Scores	Total Scores	Male Scores	Total Scores
501	19.0		25.2	
502	31.0		40.3	
503	12.0		39.1	
504	33.0		40.8	
505	43.0		33.9	
506	39.0		51.4	
510	16.4		29.9	29.8
511	29.0		40.8	
512	13.7	13.5	27.7	27.5
514	43.0		38.7	38.7
515	63.0	61.1	40.1	39.7
516	38.0		32.0	
520	25.0		33.2	
521	29.0		40.8	
522	34.0		40.6	
523	33.0		40.8	
525	50.0		38.8	
530	46.3	45.6	39.3	39.1
531	40.0		40.8	
533	22.0		36.0	
534	15.1		31.5	
535	33.0		36.8	
536	33.0		40.8	
540	34.0		35.5	
542	12.0	11.9	32.6	
543	16.9	16.8	30.7	
545	45.2		34.9	32.9
546	24.0		31.7	
550	33.7	33.4	35.6	
551	22.0	21.4	34.0	[illegible]
552	48.8		39.1	
554	49.0		39.2	
560	28.2		38.4	
561	49.2		42.3	
562	41.0		40.8	
563	21.1	21.2	29.9	
571	34.5	34.6	40.8	
572	39.0		40.8	
575	25.7	25.3	42.1	41.7
601	32.0		28.4	
602	17.2		27.5	
603	11.0		32.1	
604	18.4	16.1	23.3	23.5
605	25.0		39.4	
610	19.2	18.4	36.1	
611	17.8	16.8	21.9	20.7
612	18.8	18.9	28.8	28.5
613	23.0	22.4	31.7	31.0
614	21.6		26.2	
615	24.5	24.4	36.4	36.3

1970 Census Occupation Code	Duncan SEI		Siegel (1965 NORC) Prestige	
	Male Scores	Total Scores	Male Scores	Total Scores
620	12.0		25.0	
621	18.7	18.7	23.1	23.5
622	18.1		32.9	
623	17.9		21.2	
624	17.0	16.6	32.9	32.6
625	12.2	13.7	23.6	24.5
626	29.0		32.9	
630	15.0		18.2	
631	28.8		32.0	
633	16.4	16.2	23.6	23.5
634	18.0		19.4	
635	19.8	19.7	30.3	30.2
636	46.0		33.4	
640	16.5	16.5	26.4	
641	17.6	17.3	27.5	27.3
642	15.0		24.2	
643	18.0	18.1	19.5	
644	18.1		29.0	28.8
645	42.1	41.2	35.9	35.5
650	21.8	22.0	31.7	31.8
651	21.9		19.0	
652	21.5	21.6	31.9	31.9
653	21.0	20.9	31.1	
656	19.4	19.5	30.4	30.3
660	20.1	20.6	31.6	31.5
661	16.0		33.7	
662	04.9		27.6	
663	18.2		25.2	25.1
664	09.2		31.6	
665	23.8	24.4	35.4	36.1
666	16.6		31.7	
670	03.1	03.3	28.9	
671	21.0		29.4	
672	03.8	04.1	28.2	
673	05.9		25.0	25.1
674	06.1	08.6	28.8	
680	24.0		40.1	
681	19.6	21.7	32.0	33.6
690	19.0		28.4	28.5
692	19.3	19.3	29.3	
694	19.2	18.8	29.1	28.9
695	19.2	19.5	29.1	
701	24.0		36.8	
703	24.0	24.0	32.4	
704	32.5		28.0	
705	31.0		28.2	28.3
706	16.8	16.8	28.4	28.4
710	03.0		27.2	
711	18.8		22.0	
712	42.0		34.7	
713	44.0		32.8	
714	10.0		21.5	
715	15.1		32.1	

1970 Census Occupation Code	Duncan SEI		Siegel (1965 NORC) Prestige	
	Male Scores	Total Scores	Male Scores	Total Scores
740	16.9	17.5	28.7	30.3
750	07.2		23.0	
751	07.1	07.1	17.4	
752	10.6		30.3	
753	08.7	08.9	18.8	19.0
754	06.0	06.0	17.3	
755]	10.9		22.1	
760	11.0		24.4	
761	04.1		25.9	
762	16.7	17.3	20.6	20.7
763	08.0		12.2	
764	08.6	08.6	18.5	18.5
770	08.3		20.3	
780	08.2	08.2	19.1	
785	08.3	08.3	17.5	
801	14.0		40.7	
802	36.0		43.7	
821	20.0		35.0	
822	06.3		18.9	18.8
823	17.0		18.4	18.5
824	22.0		26.8	
901	13.4	11.6	16.6	14.1
902	07.8	09.8	18.4	17.4
903	12.7	12.5	19.5	19.3
910	19.0		19.9	
911	11.0		14.4	
912	15.0		26.4	
913	11.0		21.8	
914	17.0		15.4	
915	16.0		20.3	
916	11.0		20.9	20.8
921	38.0		47.8	
922	25.0	29.4	26.3	40.5
923	51.0		45.1	
924	37.0		23.3	
925	13.7	13.5	36.8	36.4
926	22.0		41.9	
931	31.0		36.4	
932	19.1	19.5	15.6	16.4
933	26.3	28.8	21.7	22.5
934	07.8	07.9	17.5	
935	17.0		37.9	
940	30.0		22.1	
941	08.0		09.3	
942	28.2		24.0	24.1
943	10.0		20.9	
944	17.0		33.2	
945	31.0		40.8	
950	31.0		36.4	
952	26.0		14.1	
953	25.0		14.9	
954	11.0		14.4	

ATTACHMENT 2

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THE OCCUPATIONAL SCALE

1. Higher Executives, Proprietors of Large Concerns, and Major Professionals.

a. Higher Executives

Bank Presidents; Vice-Presidents
Judges (Superior Courts)
Large Business, e.g., Directors,
  Presidents, Vice-Presidents,
  Assistant Vice-Presidents,
  Executive Secretary,
  Treasurer.

Military, Commissioned Officers, Major
and above, Officials of the Executive
  Branch of Government,
  Federal, State, Local, e.g.,
  Mayor, City Manager, City Plan
  Director, Internal Revenue
  Directors.
Research Directors, Large Firms

b. Large Proprietors (Value over $100,000).

Brokers
Contractors

Dairy Owners
Lumber Dealers

c. Major Professionals

Accountants (C.P.A.)
Actuaries
Agronomists
Architects
Artists, Portrait
Astronomers
Auditors
Bacteriologists
Chemical Engineers-[illegible]
Chemists
Clergyman (Professionally Trained)
Dentists

Economists
Engineers (College Grad.)
Foresters
Geologists
Lawyers
Metallurgists
Physicians
Physicists, Research
Psychologists, Practicing
Symphony Conductor
Teachers, University, College
Veterinarians (Veterinary Surgeons)
School Principal
Dir. Supv. [illegible]

2. Business Managers, Proprietors of Medium Sized Businesses, and Lesser Professionals.

a. Business Managers in Large Concerns.

Advertising Directors
Branch Managers
Brokerage Salesmen
District Managers
Executive Assistants
Executive Managers, Govt. Officials,
  minor, e.g., Internal Revenue Agents
Farm Managers

Office Managers
Personnel Managers
Police Chief; Sheriff
Postmaster
Production Managers
Sales Engineers
Sales Managers, National Concerns
Sales Managers (Over $100,000)
[illegible]

---

1. The value of businesses is based upon the rating of financial strength in Dun and Bradstreet's Manual.

b. Proprietors of Medium Businesses (Value $35,000-$100,000)

Advertising Owners (-$100,000)
Clothing Store Owners (-$100,000)
Contractors (-$100,000)
Express Company Owners (-$100,000)
Fruits, Wholesale (-$100,000)
Furniture Business (-$100,000)
Jewelers (-$100,000)
Labor Relations Consultants

Manufacturer's Representatives
Poultry Business (-$100,000)
Purchasing Managers
Real Estate Brokers (-$100,000)
Rug Business (-$100,000)
Store Owners (-$100,000)
Theater Owners (-$100,000)

c. Lesser Professionals

Accountants (Not C.P.A.)
Chiropodists
Chiropractors
Correction Officers
Director of Community House
Engineers (Not College Grad.)
Finance Writers
Health Educators
Librarians

Military, Commissioned Officers,
  Lts., Captains
Musicians (Symphony Orchestra)
Nurses
Opticians
Pharmacists
Public Health Officers (M.P.H.)
Research Assistants, University
  (Full-time)
Social Workers [illegible]
Teachers (Elementary and High)

3. Administrative Personnel, Small Independent Businesses, and Minor Professionals.

a. Administrative Personnel

Adjusters, Insurance
Advertising Agents
Chief Clerks
Credit Managers
Insurance Agents
Managers, Department Stores
Passenger Agents--R.R.
Private Secretaries
Purchasing Agents
Sales Representatives

Section Heads, Federal, State, and
  Local Government Offices
Section Heads, Large Businesses
  and Industries
Service Managers
Shop Managers
Store Managers (Chain)
Traffic Managers

b. Small Business Owners ($6,000-$35,000)

Art Gallery
Auto Accessories
Awnings
Bakery
Beauty Shop
Boatyard
Brokerage, Insurance
Car Dealers
Cattle Dealers

Cigarette Machines
Cleaning Shops
Clothing
Coal Businesses
Convalescent Homes
Decorating
Dog Supplies
Dry Goods
Electrical Contractors
Engraving Business

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[illegible]

Feed
Finance Co., Local
Fire Extinguishers
5 & 10
Florist
Food Equipment
Food Products
Foundry
Funeral Directors
Furniture
Garage
Gas Station
Glassware
Grocery-General
Hotel Proprietors
Inst. of Music
Jewelry
Machinery Brokers
Manufacturing

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[illegible]
[illegible]
Painting Contracting
Plumbing
Poultry Producers
Publicity & Public Relations
Real Estate
Records and Radios
Restaurant
Roofing Contractor
Shoe
Shoe Repairs
Signs
Tavern
Taxi Company
Tire Shop
Trucking
Trucks and Tractors
Upholstery
Wholesale Outlets
Window Shades

c. Semi-Professionals

Actors and Showmen
Army M/Sgt; Navy C.P.O.
Artists, Commercial
Appraisers (Estimators)
Clergymen (Not professionally trained)
Concern Managers
Deputy Sheriffs
Dispatchers, R.R. Train
I.B.M. Programmers
Interior Decorators
Interpreters, Court
Laboratory Assistants
Landscape Planners
Morticians
Oral Hygienists
Photographers
Physio-therapists
Piano Teachers
Radio, T.V. Announcers
Reporters, Court
Reporters, Newspaper
Surveyors
Title Searchers
Tool Designers
Travel Agents
Yard Masters, R.R.

d. Farmers

Farm Owners ($25,000-35,000)

4. Clerical and Sales Workers, Technicians, and Owners of Little Businesses. (Value under $6,000)

a. Clerical and Sales Workers

Bank Clerks and Tellers
Bill Collectors
Bookkeepers
Business Machine Operators, Offices
Claims Examiners
Clerical or Stenographic
Conductors, R.R.
Employment Interviewers
Factory Storekeeper
Factory Supervisor
Post Office Clerks
Route Managers (Salesmen)
Sales Clerks
Shipping Clerks
Supervisors, Utilities, Factories
Toll Station Supervisors
Warehouse Clerks

[illegible]

Camp Counselors
Dental Technicians
Draftsmen
Driving Teachers
Expeditor, Factory
Experimental Tester
Instructors, Telephone Co., Factory
Inspectors, Weights, Sanitary
  Inspectors, R.R., Factory
Investigators
Laboratory Technicians
Locomotive Engineers

Operators, P.B.X.
Proofreaders
Safety Supervisors
Supervisors of Maintenance
Technical Assistants
Telephone Co. Supervisors
Timekeepers
Tower Operators, R.R.
Truck Dispatchers
Window Trimmers (Store)

c. Owners of Little Businesses.

Flower Shop ($3,000-$6,000)
Newsstand ($3,000-$6,000)
Tailor Shop ($3,000-$6,000)

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d. Farmers.

Owners ($10,000-$20,000)

5. Skilled Manual Employees.

Adjusters, Typewriter
Auto Body Repairers
Bakers
Barbers
Blacksmiths
Bookbinders
Boilermakers
Brakemen, R.R.
Brewers
Bulldozer Operators
Butchers
Cabinet Makers
Carpenters
Casters (Founders)
Cement Finishers
Cheese Makers
Chefs
Compositors
Diemakers
Diesel Engine Repair & Maintenance
  (Trained)
Diesel Shovel Operators
Electricians
Electrotypists
Engravers
Exterminators
Fitters, Gas, Steam
Firemen, City
Firemen, R.R.
Foremen, Construction, Dairy
Gardeners, Landscape (Trained)

Glassblowers
Glaziers
Gunsmiths
Gauge Makers
Hair Stylists
Heat Treaters
Horticulturists
Lineman, Utility
Linoleum Layers (Trained)
Linotype Operators
Lithographers
Locksmiths
Loom Fixers
Lumberjacks
Machinists (Trained)
Maintenance Foremen
Installers, Electrical Appliances
Masons
Masseurs
Mechanics (Trained)
Millwrights
Moulders (Trained)
Painters
Paperhangers
Patrolmen, R.R.
Pattern and Model Makers
Piano Builders
Piano Tuners
Plumbers
Policemen, City TA [illegible]
Postmen

[illegible]
Radio, T.V., Maintenance
[illegible], Home Appliances
Riggers
Rope Splicers
Sheet-metal Workers (Trained)
Shipsmiths
Shoe Repairmen (Trained)
Stationary Engineers (Licensed)
Stewards, Club
Switchmen, R.R.
[illegible])
[illegible]
[illegible]
Track Supervisors, R.R.
Tractor-Trailer Trans.
Typographers
Upholsterers (Trained)
Watchmakers
Weavers
Welders
Yard Supervisors, R.R.

Small Farmers

Owners (under $10,000)
Tenants who own farm equipment

6. Machine Operators and Semi-Skilled Employees

Aides, Hospital
Apprentices, Electricians, Printers
Steamfitters, Toolmakers
Assembly Line Workers
Bartenders
Bingo Tenders
Building Superintendents (Cust.)
Bus Drivers
Checkers
Clay Cutters
Coin Machine Fillers
Cooks, Short Order
Delivery Men
Dressmakers, Machine
Drill Press Operators
Duplicator Machine Operators
Elevator Operators
Enlisted Men, Military Services
Filers, Benders, Buffers,
Foundry Workers
Garage and Gas Station Assistants
Greenhouse Workers
Guards, Doorkeepers, Watchmen
Hairdressers
Housekeepers
Meat Cutters and Packers
Meter Readers
Operators, Factory Machines
Oiler, R.R.
Paper Rolling Machine Operators
Photostat Machine Operators
Practical Nurses
Pressers, Clothing
Pump Operators
Receivers and Checkers
Roofers
Set-up Men, Factories
Shapers
Signalmen, R.R.
Solderers, Factory
Sprayers, Paint
Steelworkers (Not Skilled)
Stranders, Wire Machines
Strippers, Rubber Factory
Taxi Drivers
Testers
Timers
Tire Moulders
Trainmen, R.R.
Truck Drivers, General
Waiters-Waitresses ("Better Places")
Weighers
Welders, Spot
Winders, Machine
Wiredrawers, Machine
Wine Bottlers
Wood Workers, Machine
Wrappers, Stores and Factories
Route men for newspaper

Farmers

Smaller Tenants who own little equipment.

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7. Unskilled Employees.

Amusement Park Workers (Bowling
  Alleys, Pool Rooms)
Ash Removers
Attendants, Parking Lots
Cafeteria Workers
Car Cleaners, R.R.
Car Helpers, R.R.
Carriers, Coal
Countermen
Dairy Workers
Deck Hands
Domestics
Farm Helpers
Fishermen (Clam Diggers)
Freight Handlers
Garbage Collectors
Grave Diggers
Hod Carriers
Hog Killers
Hospital Workers, Unspecified
Hostlers, R.R.
Janitors, Sweepers
Laborers, Construction
Laborers, Unspecified
Laundry Workers
Messengers
Platform Men, R.R.
Peddlers
Porters
Roofer's Helpers
Shirt Folders
Shoe Shiners
Sorters, Rag and Salvage
Stagehands
Stevedores
Stock Handlers
Street Cleaners
Unskilled Factory Workers
Truckmen, R.R.
Waitresses--"Hash Houses"
Washers, Cars
Window Cleaners
Woodchoppers

Relief, Public, Private

Unemployed (No Occupation)

Farmers

Share Croppers

0 = Student

This scale is premised upon the assumption that occupations have different values attached to them by the members of our society. The hierarchy ranges from the low evaluation of unskilled physical labor toward the more prestigeful use of skill, through the creative talents of ideas, and the manipulation of men. The ranking of occupational functions implies that some men exercise control over the occupational pursuits of other men. Normally, a person who possesses highly trained skills has control over several other people. This is exemplified in a highly developed form by an executive in a large business enterprise who may be responsible for decisions affecting thousands of employees.

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The Educational Scale

The educational scale is premised upon the assumption that men and women who possess similar educations will tend to have similar tastes and similar attitudes, and they will also tend to exhibit similar behavior patterns. The educational scale is divided into seven positions: (1) *Graduate Professional Training*. (Persons who complete a recognized professional course leading to a graduate degree are given scores of 1). (2) *Standard College of University Graduation*. (All individuals who complete a four-year college or university course leading to a recognized college degree are assigned to same scores. No differentiation is made between state universities or private colleges.) (3) *Partial College Training*. (Individuals who complete at least one year but not a full college course are assigned this position. Most individuals in this category complete from one to three years of college.) (4) *High School Graduates*. (All secondary school graduates whether from a private preparatory school, a public high school, a trade school, or a parochial high school, are assigned the same scale value.) (5) *Partial High School*. (Individuals who complete the tenth or the eleventh grades, but do not complete high school are given this score.) (6) *Junior High School*. (Individuals who complete the seventh grade through the ninth grade are given this position.) (7) *Less Than Seven Years of School*. (Individuals who do not complete the seventh grade are given the same scores irrespective of the amount of education they receive.)

Integration of Two Factors

The factors of *Occupation* and *Education* are combined by weighing the individual scores obtained from the scale positions. The weights for each factor were determined by multiple correlation techniques. The weight for each factor is:

Factor	Factor Weight
Occupation	7
Education	4

To calculate the Index of Social Position score for an individual, the scale value for Occupation is multiplied by the factor weight for Occupation, and the scale value for Education is multiplied by the factor weight for Education. For example, John Smith is the manager of a chain supermarket. He completed high school and one year of college. His Index of Social Position score is computed as follows:

Factor	Scale Score	Factor Weight	Score X Weight
Occupation	3	7	21
Education	3	4	12
	Index of Social Position Score		33

Index of Social Position Scores*

The Two Factor Index of Social Position Scores may be arranged on a continuum, or divided into groups of scores. The range of scores on a continuum is from a low of 11 to a high of 77. For some purposes a researcher may desire to work with a continuum of scores. For other purposes he may desire to break the continuum into a hierarchy of score groups.

I have found the most meaningful breaks for the purpose of predicting the social class position of an individual or of a nuclear family is as follows:

Social Class	Range of Computed Scores
I	11-17
II	18-27
III	28-43
IV	44-60
V	61-77

---

* See August B. Hollingshead and Frederick C. Redlich, Social Class and Mental Illness, John Wiley and Sons, New York, 1958, pp. 398-407.

3/30/79

Consortium for Longitudinal Studies

1979 Youth Interview

Hello. Is this Mr./Miss (first and last name)? This is ____________. I'm from (name of university or project). I sent you a letter (a while ago), and as I mentioned, I am calling to ask you some questions. As you know, we are trying to learn more about people like yourself. I would like to ask you about some things that have happened since the last time we talked.

Everything you tell me is completely confidential. Your name will not be on any forms, so no one can connect your name with your answers. It's important that you feel free to tell me exactly what you think and not just what you feel I want to hear. If there are questions that aren't clear to you or you feel are too personal, please tell me.

Let me remind you we are not part of the public schools or the government.

Project ID ☐☐ Child ID ☐☐☐☐ File [9] Card [1] 1-

[NOTE: RECORD 9, 99, FOR NO ANSWER, NOT APPLICABLE;
RECORD 8, 98, FOR "DON'T KNOW"]

1. First I want to ask some questions about your household. Could you list for me everyone who lives in your household at the present time, starting with the oldest person and going down to the youngest.

[NOTE: DO NOT INCLUDE RESPONDENT IN LISTING]

[AFTER EVERYONE IS LISTED, GO BACK AND FOR EACH PERSON ASK:]

1a. What relation is (person) to you?

1b. [IF NOT OBVIOUS BY NAME] Is that a man (boy) or woman (girl)?

1c. How old is s/he?

Code for Relationship

01 father
02 mother
03 guardian/step-parent
04 grandparent
05 uncle/aunt
06 gr. uncle/aunt
07 sibling
08 hf/step sibling
09 cousin
10 nieces/nephews
11 own child
12 husband/wife
13 boy/girlfriend (marriage-type relationship)
14 no relation
15 other (specify)

	(name or initial)	a. Relation (see code)	b. Sex 1 Male 2 Female	c. Age (yrs. at last birthday)	
1	________	☐☐	☐	☐☐	9-
2	________	☐☐	☐	☐☐	14-
3	________	☐☐	☐	☐☐	19-
4	________	☐☐	☐	☐☐	24-
5	________	☐☐	☐	☐☐	29-
6	________	☐☐	☐	☐☐	34-
7	________	☐☐	☐	☐☐	39
8	________	☐☐	☐	☐☐	44
9	________	☐☐	☐	☐☐	49
10	________	☐☐	☐	☐☐	54
11	________	☐☐	☐	☐☐	59
12	________	☐☐	☐	☐☐	64

13 If more than 12 people in household, record total number in col. 69 - 70 and attach additional household sheets. ☐☐ 69

Child ID ____________

* * * * * * * 2 1-8

[IF RESPONDENT'S SON(S) OR DAUGHTER(S) ARE NAMED, ASK:]
What month and year was (child) born?

1d.

	birth month	birth year	
(firstborn child)	☐☐	☐☐	9-12
(secondborn child)	☐☐	☐☐	13-16
(thirdborn child)	☐☐	☐☐	17-20
(fourthborn child)	☐☐	☐☐	21-24

2a. Are you currently: married, living with someone, divorced, separated, widowed, or have you never been married? ☐ 25

1 married
2 living with companion in marriage-type relationship
3 divorced
4 separated (married, not living with husband/wife)
5 widowed
[ASK #2b]
6 never been married [GO TO #4]
7 other, specify ____________________

2b. How long have you been (married) (living together) (separated) (widowed) (divorced)? ☐☐ months 26-27

[IF MARRIED, DIVORCED, SEPARATED, WIDOWED, ASK:]
2c. Is this (was that) your first marriage?

[IF YES, RECORD 1 IN #2d AND SKIP TO #3a]
[IF NO, ASK #2d]

2d. How many times have you been married? (0=never; 1=once; 2=twice; etc.) ☐ 28

[IF LIVING WITH SOMEONE NOW, ASK:]
2e. (alternate) Have you ever been married?

[IF NO, RECORD 0 IN #2d AND SKIP TO #3a]
[IF YES, ASK #2d ABOVE]

[ASK ABOUT PRESENT HUSBAND/WIFE COMPANION:]

3a. What is highest grade your husband/wife (companion) completed in school and got credit for? (13=1 yr.post-high school, etc.) ☐☐ 29-30

[IF 12 OR MORE YEARS, ASK:]

3b. What degree or diploma did s/he receive?

1 none	4 trade school		
2 high school	5 4 year college	☐	31
3 2 year college	6 other (specify)____________		

3c. At present, what is s/he doing? Is s/he working full or part time, going to school, keeping house, or what? ☐☐ 32-33

01 in school [SKIP TO #3g]
02 working full-time at present
03 working part-time at present [ASK #3d]
04 working and in school [ASK #3d,e,f,g]
05 not working and not in school
06 keeping house [SKIP TO #4]
07 in armed services
0[illegible], specify______________________________

Office Use

3d. What kind of work does s/he do? What is his/her main job called? ☐ 34

____

3e. Exactly what does s/he do in that job? What are his/her main duties? ☐☐☐ 35-37

____

3f. What kind of business or industry is s/he in? What do they do or make at the place where s/he works? ☐☐☐ 38-40

____

[IF SPOUSE IS IN SCHOOL, ASK:]

3g. What kind of educational program is s/he attending? ☐☐ 41-42 ☐☐ 43-44

01 still in high school
02 high school equivalency program
03 vocational school (e.g., business training, secretarial school), specify__________________
04 on-job-training
05 junior college
06 college
07 graduate school
08 armed services (specify: active, reserve national guard)________________________
09 other, specify (e.g., prison, residential center)________________________
10 other, specify________________________

Now I'd like to talk about *your* school experiences.

4a. What is the highest grade in elementary school, junior or senior high school that you finished and got credit for? (09=9th grade; 12=12th grade; etc.) ☐☐ 43-46

4b. Did you get a high school diploma or pass an equivalency test? ☐ 47

1 no
2 equivalency test (G.E.D.)
3 high school diploma

5a. Are you in school or any educational program now like college, vocational school, or on-the-job training? ☐ 48

1 no, not in school or any program now [SKIP TO #6]
2 yes [ASK #5b]

5b. What kind of program(s) are you in? ☐☐ 49-50 ☐☐ 51-52

01 still in high school
02 high school equivalency program
03 vocational school (e.g., business training, secretarial school), specify______________
04 on-job-training
05 junior college
06 college
07 graduate school
08 armed services (specify: active, reserve, national guard)______________
09 other, specify (e.g., prison, residential center)______________
10 other, specify______________

[PROBE: IF PROGRAM NAMED BUT TYPE NOT OBVIOUS, ASK:]

What is the purpose of that program?

[PROBE: IF SAYS "COLLEGE", ASK:]

Is that a 4-year college, 2-year college, vocational school or what?

6a. Have you ever attended any (any other) educational or training program since high school? ☐ 53

1 no [SKIP TO #7a]
2 yes [ASK #6b]

6b. What kind of school(s) or program(s) were you in? ☐☐ 54-55

☐☐ 56-57

01 high school equivalency progam
02 vocational school (e.g., business training, secretarial school), specify__________
03 on-job-training
04 junior college
05 college
06 graduate school
07 armed services (specify: active, reserve, national guard)__________
08 other, specify (e.g., prison, residential center)__________
09 other, specify __________

[PROBE: IF PROGRAM IS NAMED BUT TYPE IS NOT OBVIOUS, ASK:]

What is the purpose of (that program)?

6c. Did you complete the program (graduate)? ☐ 58

1 no [ASK #6d]
2 yes

6d. Why did you stop attending?__________
____

Office Use
☐☐ 59-60
☐☐ 61-62

7a. How many years of schooling beyond high school have you completed for credit? Include vocational training, armed services, night school, etc. [ADD TOGETHER ALL SEPARATE EDUCATIONAL EXPERIENCES]

0 none
1 1 year or less
2 up to 2 years
3 up to 3 years
4 etc.

☐ 63

7b. Do you have any certificate, licenses or college degrees? [RECORD ALL POSSIBLE RESPONSES]

1 no
2 certificates, specify________________________
3 licenses, specify________________________
4 junior college associate degree
5 bachelor's degree
6 graduate degree, specify________________________

☐ 64
☐ 65

[IF DROPPED OUT OF HIGH SCHOOL, ASK:]

8. People have all kinds of reasons for leaving high school before they graduate. Could you tell me what your reasons were?

____

Office Use
☐☐ 66-67

8a. Did you ever return to high school at any time, even for a short period?

1 no [SKIP TO #9]
2 yes [ASK #8b]

☐ 68

8b. How many times did you return to school? (1=once; 2=twice, etc.)

☐ 69

8c. Why did you decide to return to school?

____
____

Office Use
☐☐ 70-71
☐☐ 72-73

9. If you could go as far as you would like to go in school, how far would that be? ☐☐ 74-75

- 00 gone as far as wants to go [SKIP TO #11]
- 01 finish regular high school
- 02 finish high school equivalency program
- 03 finish vocational school
- 04 some college
- 05 complete 2-year college
- 06 complete college
- 07 graduate or professional training
- 08 other ______________________________
- 10-17 as far as I can go [PROBE FOR AMOUNT: RECORD 10 IF AMOUNT NOT SPECIFIED, 11 FOR REGULAR HIGH SCHOOL, 12 FOR HIGH SCHOOL EQUIVALENCY PROGRAM, ETC.]

10a. How far do you think you will really be able to go in school? ☐☐ 76-77

- 00 no plans to go any further [SKIP TO #11]
- 01 finish regular high school
- 02 finish high school equivalency program
- 03 finish vocational school
- 04 some college
- 05 complete 2-year college
- 06 complete college
- 07 graduate or professional training
- 08 other, specify ______________________________
- 10-17 as far as I can go [PROBE FOR AMOUNT: RECORD 10 IF AMOUNT NOT SPECIFIED, 11 FOR REGULAR HIGH SCHOOL, 12 FOR HIGH SCHOOL EQUIVALENCY PROGRAM, ETC.]

[IF NOT PRESENTLY ENROLLED, ASK:]

10b. What are your plans for (doing that)?

____

____

[IF DISCREPANCY BETWEEN #9 AND #10, ASK:]

10c. Why do you think you will have (less)(more) schooling than you'd like to have?

____

Office Use

☐☐ 78-79

| * | * | * | * | * | * | * | 3 | 1-8

☐☐ 9-10

☐☐ 11-12

11. This is an opinion question. Some people say it's important to have a high school education, others say it's not important. Do you think a high school education is: [READ ALTERNATIVES] ☐ 13

- 1 very important
- 2 somewhat important
- 3 no opinion one way or another
- 4 somewhat unimportant
- 5 not important at all?

[ASK EVERYONE]

12. Many people for all kinds of reasons are asked to leave school. Were you ever suspended or expelled? ☐ 14

1 never suspended or expelled [SKIP TO #13]
2 suspended only [ASK #12a]
3 expelled only [SKIP TO #12h]
4 both suspended & expelled [ASK #12a]

12a. Were you suspended more than once? [IF NO, RECORD 01 IN 12a] [IF YES, ASK:] How many times were you suspended? (2=twice; 3=three times; etc.) ☐☐ 15-16

12b. What grade was that (first time)? (08=8th grade) ☐☐ 17-18

12c. What was the reason (what happened)? ______________________ Office Use ☐☐ 19-20

12d. What grade was (second time)? ☐☐ 21-22

12e. What was the reason (what happened)? ______________________ Office Use ☐☐ 23-24

12f. What grade was (latest time)? ☐☐ 25-26

12g. What was the reason (what happened)? ______________________ Office Use ☐☐ 27-28

12h. Were you expelled more than once? [IF NO, RECORD 1] [IF YES, ASK:] How many times were you expelled? (1=once; 2=twice; etc.) ☐ 29

12i. What grade were you when you were expelled (first time)? ☐☐ 30-31

12j. What was the reason (what happened)? ______________________ Office Use ☐☐ 32-33

[IF EXPELLED MORE THAN ONCE, ASK:]
12k. What was the reason for the last time? ______________________ ☐☐ 34-35

13. Now I'd like to ask you about jobs.

13a. Are you working for pay now either full-time or part-time? ☐ 36

1 no [ASK #13b]
2 yes [SKIP TO #13c]

13b. Have you *ever* had a full-time or part-time job? ☐ 37

1 no [SKIP TO #13f]
2 yes [ASK #13c]

13c. How old were you when you got your first full-time job? (yrs) ☐☐ 38-39

How old were you when you got your first part-time job? (yrs) ☐☐ 40-41

13d. How many different full-time jobs have you had? ☐☐ 42-43

How many different part-time jobs have you had? ☐☐ 44-45

[IF WORKING NOW, SKIP TO #14. IF NOT WORKING NOW, ASK:]

13e. When did you last work for pay at a regular job either full-time or part-time? ☐ 46

1 within past 12 months
2 1 to 2 years ago
3 2 to 3 years ago
4 more than 3 years ago
[ASK #13f]

13f. Would you like to be working or would you rather not work at this time? ☐ 47

1 would like to work [ASK #13g]
2 rather not work [ASK #13g]

13g. Tell me, what are some of the reasons why you (are not working) (would rather not work)?

____
____
____

Office Use
☐☐ 48-49
☐☐ 50-51

Child ID________

[IF NEVER WORKED, SKIP TO #16]

[ASK ABOUT PRESENT (LAST) JOB:]

Office Use

14. What kind of work do (did) you do?
[PROBE: What is (was) your main job called?]
[PROBE: What is (was) the title of your job?]

______________________________ ☐ 52

14a. Exactly what do (did) you do in that job? What are (were) your main duties?

______________________________ ☐☐☐ 53-

14b. What kind of business or industry is (was) that in? What do (did) they do or make at the place where you work (worked)?

______________________________ ☐☐☐ 56-

14c. How long have you been working (did you work) there? ☐☐ 59-60 months

14d. How many hours a week do (did) you work there? ☐☐ 61-62

14e. Some people are paid for the number of hours they actually work. Others are paid a set salary for the month or year. Do (did) you get an hourly wage based on the number of hours you actually work(ed) or do (did) you receive a set salary for working full or part-time? [PROBE:] Do (did) you punch a time clock or fill out a time sheet by the hour?

1 paid an hourly wage
2 set salary for month or year ☐ 63

14f. What is (was) your pay per hour? $☐☐.☐☐ 64-6 $/hour

[OR]

What is (was) your salary for the $______ month; $______ year

14g. Do you (did you) have any other jobs besides your main job or do you (did you) do any other work for pay? ☐ 68

1 no
2 yes [ASK:] What else did you do?

____

272

14h. Overall, how satisfied are you (were you) with your (main) job? Would you say you are (were):

1 very satisfied
2 somewhat satisfied
3 no feeling one way or another
4 somewhat dissatisfied
5 very dissatisfied

☐ 69

Office Use

14i. What do (did) you like about your job?

____
____

☐☐ 70-71
☐☐ 72-73

14j. What don't (didn't) you like about your job?

____
____

☐☐ 74-75
☐☐ 76-77

[IF DESCRIBED FORMER JOB, ASK:]

14k. Why did you leave that job?

____

☐☐ 78-79

||*|*|*|*|*|4 1-8

15. Did you have a part-time or full-time job before this (that) one?

1 no [SKIP TO #16]
2 yes [ASK #15a]

☐ 9

15a. What kind of work did you do in that job? What was your main job called? What was the title of your job?

____

Office Use

☐ 10

[PROBE:]

15b. Exactly what did you do in that job? What were your main duties?

____

☐☐☐ 11-13

15c. What kind of business or industry was that in? What did they do or make at the place where you worked?

____

☐☐☐ 14-16

15d. How long did you work there?

☐☐ months 17-18

15e. How many hours a week did you work? ☐☐ 19-20

15f. Did you get an hourly wage based on the number of hours you actually worked or did you receive a set salary for the month or year?

1 paid an hourly wage
2 set salary for month or year

☐ 21

15g. What was your pay per hour? $☐☐.☐☐ $/hour 22-25

[OR]

What was your salary for the $________ month; $________ year

15h. Overall, how satisfied were you with that job? Would you say you were: [READ ALTERNATIVES]

1 very satisfied
2 somewhat satisfied
3 no feelings one way or another
4 somewhat dissatisfied
5 very dissatisfied

☐ 26

15i. What did you like about that job?

____

15j. What didn't you like about that job?

____

15k. Why did you leave that job?

____

Office Use
☐☐ 27-28
☐☐ 29-30
☐☐ 31-32
☐☐ 33-34
☐☐ 35-36
☐☐ 37-38

16. What plans do you have for work in the next six months or so?
[PROBE:] Are you looking for work (other work) now?

[RECORD AS MANY RESPONSES AS APPLICABLE]

☐☐ 39-40
☐☐ 41-42
☐☐ 43-44

01 no particular plans [PROBE, BUT IF STILL NO RESPONSE, SKIP TO #17]
02 no plans to look for work (or other work) [SKIP TO #17]
03 continue in school [SKIP TO #17]
04 looking for work at present [ASK #16a]
05 looking for different or additional job [ASK #16a]
06 planning to look for work in next 6 months [ASK #16a]
07 get more training [ASK #16c]
08 start school [ASK #16c]
09 have other job lined up [ASK 16a]
10 other, specify____________________

16a. What type of work are you looking for (will you look for)(do you have lined up)?

____

Office Use
☐☐ 45-46

16b. How (are you going) (will you go) about finding (other) work? [SKIP TO #17]

1 friends, relatives are helping, specify how____________________
2 employment agencies
3 reading ads
4 going door-to-door
5 other, specify____________________

☐ 47

16c. What kind of training do you want? (Or what kind of school will you be starting?)

____

Office Use
☐ 48
☐ 49

17. Now think about the future for a moment -- like 5 or 10 years from now. Ideally, if you could choose, what kind of work would you *like* to be doing in the future?

____

____

☐☐☐ 50-52

17a. Now imagine it is 1989, ten years from now. What kind of job do you think you will actually have?

____

____

☐ 53
☐☐☐ 54-56

18. Now I'd like to ask you some questions about money. People have different ways to earn money. We are not interested in the actual amount of money you receive, but we want to ask how you are supporting yourself. I want to remind you that everything you tell me is confidential. Your name is not on any forms and no one can connect your name with your answers. The information is only for our own study.

18a. At present, do you (or your wife/husband) receive any money from [READ ALL APPROPRIATE ALTERNATIVES]?

1 no
2 yes
9 not applicable

A. your own work	☐	57
B. spouse's or companion's work	☐	58
C. your parents (or your spouse's/companion's parents)	☐	59
D. other relatives	☐	60
E. the father of your child(ren)	☐	61
F. veteran benefits	☐	62
G. social security	☐	63
H. public assistance or welfare (like AFDC,SSI)	☐	64
I. unemployment compensation	☐	65
J. scholarship, financial aid, etc.	☐	66
K. any other sources? (specify)____________________	☐	67

18b. You said your income comes from [REPEAT ALL SOURCES IN 18a]. Which source does most of your money come from? Where does the second largest amount of income come from? What is the third largest? What is the next largest source of income? [RANK SOURCES USING CODE BELOW]

01 your own work
02 spouse's or companion's work
03 your parents (or your spouse's/companions parents)
04 other relatives
05 the father of your child(ren)
06 veteran benefits
07 social security
08 public assistance
09 unemployment compensation
10 scholarship, financial aid, etc.
11 other sources, specify____________________

☐☐	most income	68-69
☐☐	2nd largest	70-71
☐☐	3rd largest	72-73
☐☐	4th largest	74-75

* * * * * * * 5 1-8

19. (Would you please look at this card which lists some job characteristics) OR (I'm going to read you a list of some job characteristics). I'm going to ask you to rank them from the one you most prefer in a job to the one that is least important. [HAND CARD TO R]

19a. Tell me which one item on this list is most important to you in a job? [READ NUMBER AND ALL CHOICES] ☐ 9

1 high income
2 no danger of being fired
3 short working hours so you have lots of free time
4 chances for advancement or promotion
5 important work that gives a feeling of accomplishment

19b. Which is the next most important to you? [USE CODE IN 19a] [IF NO CARD, READ REMAINING CHOICES] ☐ 10

19c. Which is third most important? [USE CODE IN 19a] [IF NO CARD, READ REMAINING CHOICES] ☐ 11

19d. Of the last two items, which is more important? [USE CODE IN 19a] ☐ 12

19e. [IDS ONLY:] [RECORD WHICH ITEM IS LEAST IMPORTANT] ☐ 13

20. If you were to get enough money to live as comfortably as you would like for the rest of your life (like winning the lottery), would you continue to work or would you stop working? ☐ 14

1 continue to work
2 stop working
3 don't know
4 other, specify____

[ASK BOTH MALES AND FEMALES:]

21. Now I want to ask you about personal relationships. First we want to know about children (other children).

[IF CHILDREN MENTIONED, ASK:] Do you have any other children of your own who are not living with you?

[IF CHILDREN NOT MENTIONED, ASK:] You didn't mention any children of your own. Do you have any children of your own who are not living with you?

1 no [SKIP TO #22]
2 yes [ASK #21a] ☐ 15

21a. How many? ☐ 16

21b. [FOR EACH CHILD ASK:]
Is it a boy or girl?
What month and year was (child) born?
Who does (child) live with?

	Sex 1 male 2 female	Birth Month	Birth Year	Who does child live with? (use code below)	
(first child)	☐	☐☐	☐☐	☐☐	17-23
(second child)	☐	☐☐	☐☐	☐☐	24-30
(third child)	☐	☐☐	☐☐	☐☐	31-37
(fourth child)	☐	☐☐	☐☐	☐☐	38-44

01 child's father/mother
02 child's grandparents
03 other relative of child, specify ____________
04 friend, neighbor
05 foster parent
06 other ____________

[ASK MALES AND FEMALES:]

22. What do you think is the ideal number of children, if any, you'd like to have? ☐☐ 45-46
(00=no children; 01=1 child, etc.)

23. How many children do you think you will actually end up having? ☐☐ 47-48
(00=no children; 01=1 child, etc.)

[FEMALES ONLY, FOR MALES SKIP TO #25]

24. Are you pregnant now or have you ever been pregnant?
(You told me about the children you have already. Are you pregnant now?)

1 never pregnant [SKIP TO #25]
2 not pregnant now, but pregnant previously ⎤
3 pregnant now for the first time ⎥ [ASK #24a] ☐ 49
4 pregnant now and pregnant previously ⎦

24a. How many times have you been pregnant? ☐ 50
(1=once; 2=twice; etc.; 7=7 or more times)

24b. Many women have pregnancies and lose the baby for some reason like miscarriages. Has this ever happened to you? ☐ 51

0 no miscarriages [ASK #24c]

[IF YES, ASK:] How many times? [ASK #24c]
(1=once; 2=twice; etc.; 7=7 or more times)

24c. Some women get pregnant and then do something to end the pregnancy or have an abortion. Have you ever had a pregnancy end this way? ☐ 52

0 no
[IF YES, ASK:] How many times?
(1=once; 2=twice; etc.; 7=7 or more times)

[ASK MALES AND FEMALES]

25. There are several types of birth control methods. Can you name any of them? [PROBE:] Do you know any others?

☐☐ 53-54
☐☐ 55-56
☐☐ 57-58
☐☐ 59-60
☐☐ 61-62
☐☐ 63-64

00 no, can't name any methods
01 pill
02 condom, rubber
03 foam, syringes, suppositories, jelly
04 diaphragm
05 IUD, CU7
06 rhythm
07 withdrawal
08 douche
09 other, specify____

25a. How did you find out about these methods? [PROBE]

____

____

Office Use
☐☐ 65-66

25b. Now I want to ask about your use of birth control methods. Would you say that you [READ ALTERNATIVES:]

1 never had sexual relations] [SKIP TO #26]
2 never used birth control methods] [SKIP TO #26]
3 or that you or a partner have used birth control [ASK #25c]

☐ 67

25c. At the present time how regularly would you say you or a partner use birth control methods? [READ ALTERNATIVES]

1 always
2 sometimes
3 rarely, almost never
4 never

☐ 68

25d. Was a birth control method used the last time you had sexual relations?
1 no [SKIP TO #26]
2 yes [ASK #25e]

☐ 69

→ 25e. Which method did you use?

____

____

Office Use
☐☐ 70-71

Child ID______________

| * | * | * | * | * | * | * | 6 | 1-8

[GIVE QUESTION 26 SHEETS TO R AND READ ITEMS TO R]

26. For each of the following statements, please tell me whether you strongly agree, agree, disagree, or strongly disagree that the statement applies to you.

26a. On the whole, I am satisfied with myself. ☐ 9

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26b. At times I think I am no good at all. ☐ 10

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26c. I feel that I have a number of good qualities. ☐ 11

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26d. I feel I do not have much to be proud of. ☐ 12

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26e. I am able to do things as well as most other people. ☐ 13

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26f. I certainly feel useless at times. ☐ 14

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26g. I feel that I'm a worthwhile person, at least as much as others. ☐ 15

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26h. I wish I could have more respect for myself. ☐ 16

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26i. I try to think well of myself. ☐ 17

1 strongly agree
2 agree
3 disagree
4 strongly disagree

26j. All in all, I often feel that I am a failure. ☐ 18

1 strongly agree
2 agree
3 disagree
4 strongly disagree

Office Use

27. Tell me something you've done that made you feel proud of yourself.

____

____

☐☐ 19-20

☐☐ 21-22

28. Everyone has problems when they are growing up. What were your most serious problems when you were growing up?

____

____

☐☐ 23-24

☐☐ 25-26

29. Did you have people who were helpful to you when you were growing up? [PROBE IF NECESSARY, Who?] How were they helpful to you?

____

____

☐☐ 27-28

☐☐ 29-30

30. Everybody gets hassled by other people sometimes. On this list that I show you (read to you), let me know which of these people or places have been giving you a hard time lately. [SHOW CARD AND READ ALL APPROPRIATE ALTERNATIVES]

01 Supervisor at work

02 People you work with

03 Social worker (welfare worker)

04 School (teachers or other people who run school)

05 Somebody in your family

06 The police or the courts

07 Probation or parole officers

08 Your church (or people at your church)

09 Boyfriends or girlfriends

10 Other friends

11 Any gangs

12 Lawyers

13 Anybody you owe money to or owes you money

14 Storekeepers

15 A Hospital or people that work in an hospital or clinic like doctors or clerks

16 Neighbors, people you know in your neighborhood

17 Anyone else? Specify________________________________

20 No one has been hassling me

☐☐ 31-32
☐☐ 33-34
☐☐ 35-36
☐☐ 37-38
☐☐ 39-40
☐☐ 41-42
☐☐ 43-44
☐☐ 45-46

Supervisor:
Remove ID#
CID __________

31. [LONG FORM]

Now, I want to ask you about activities which may be against the rules or against the law. I hope you will answer all of these questions. However, if you find a question which you cannot answer honestly, tell me that you prefer not to answer that question and we will go on to the next question. Remember, your answers will never be connected with your name.

[FOR THIS SECTION, ASK: Have you ever...? IF RESPONDENT SAYS no, RECORD 0 AND GO ON TO NEXT QUESTION. IF RESPONSE IS YES, ASK:] How many times?

Have you ever...

a. Argued or had a fight with either of your parents? ☐ 47

0 never
[IF YES, ASK:]
How many times?

1 = once
2 = twice
3 = three times
4 = four times, etc.

7 = 7 or more times
8 = don't know, refused to answer
9 = not applicable

b. Hit an instructor or supervisor? [CODE AS IN 31a] ☐ 48

c. Gotten into a serious fight in school or at work? [CODE AS IN 31a] ☐ 49

d. Taken part in a fight where a group of your friends were against another group? [CODE AS IN 31a] ☐ 50

e. Hurt someone badly enough to need bandages or a doctor? [CODE AS IN 31a] ☐ 51

f. Used a knife or gun or some other thing (like a club) to get something from a person? [CODE AS IN 31a] ☐ 52

g. Taken something not belonging to you worth under $50? [CODE AS IN 31a] ☐ 53

h. Taken something not belonging to you worth over $50? [CODE AS IN 31a] ☐ 54

i.	Taken something from a store without paying for it? [CODE AS IN 31a]	☐	55
j.	Taken a car that didn't belong to someone in your family without permission of the owner? [CODE AS IN 31a]	☐	56
k.	Taken part of a car without permission of the owner? [CODE AS IN 31a]	☐	57
l.	Gone into some house or building when you weren't supposed to be there? [CODE AS IN 31a]	☐	58
m.	Set fire to someone's property on purpose? [CODE AS IN 31a]	☐	59
n.	Engaged in sexual activity for money? [CODE AS IN 31a]	☐	60
o.	Damaged school property on purpose? [CODE AS IN 31a]	☐	61
p.	Damaged property at work on purpose? [CODE AS IN 31a]	☐	62
q.	Used any drugs to get high or for kicks, except marijuana? [CODE AS IN 31a]	☐	63

32. Now let's talk about your free time. At the present, do you belong to any religious groups, labor unions, sports teams, clubs in school or the neighborhood, or anything like that? ☐ 9

1 no [SKIP TO #33]
2 yes [ASK #32a]

32a. What groups do you belong to? ☐☐ 10-11

☐☐ 12-13
☐☐ 14-15

01 church or religious group
02 labor union
03 sports team
04 gang (name)
05 music group
06 community group
07 other________________________________

08 other________________________________

33. Did you ever do any volunteer work, helping people outside your home for which you were not paid? ☐ 16

1 no
2 yes, specify________________________________ ☐☐ 17-18

34. I want to ask you about some agencies and services that you may have used.

Have you yourself ever used or had contact with: [HAND CARD AND READ EVERY ALTERNATIVE]

	1=no; 2-yes	
a. employment services	☐	19
b. job training programs; CETA	☐	20
c. unemployment compensation	☐	21
d. food stamps	☐	22
e. legal aid	☐	23
f. public assistance or welfare (e.g. AFDC)	☐	24
g. family planning clinic	☐	25
h. social security office	☐	26
i. foster care services	☐	27
j. any other agencies or services, (like veterans administration, community action agencies -- like equal opportunity centers -- or any other sources of help) (specify)______________________	☐	28
k. other (specify)________________	☐	29

35. This is the last question. Here is a list of areas of life. Most of these things are important to all of us, but each person feels that some of these things are more important than others. [PROBE TO OBTAIN THE 3 MOST AND THE 3 LEAST IMPORTANT AREAS] Look over the list and tell me:

35a. What 3 areas are most important to you? [HAND CARD AND READ LIST] [RECORD 1 FOR THE 3 MOST IMPORTANT AREAS; RECORD 2 FOR THE 3 LEAST IMPORTANT; RECORD 3 FOR THE 3 NOT MENTIONED]

35b. Now, what 3 areas are least important to you?

- a good family life	☐	30
- having money and financial security	☐	31
- things you like to do when you are not working -- hobbies and things like that	☐	32
- an interesting job	☐	33
- having good friends, and the right number of friends	☐	34
- being in good health and good physical condition	☐	35
- having a strong religious faith	☐	36
- self-understanding/personal growth & development	☐	37
- a house or apartment in a neighborhood that you like to live in	☐	38

Child ID____________

36. Interviewer signature______________________________ Interviewer ID#______________

37. Date of interview

month ☐☐ day ☐☐ year ☐☐ 39-44

38. Interviewer sex ☐ 45

0 male
1 female

39. Interviewer ethnic origin ☐ 46

0 Caucasian
1 Black
2 Hispanic
3 other, specify__________________________________

40. Duration of interview (in minutes) ☐☐ 47-48

41. Place of interview ☐ 49

0 telephone
1 home of respondent
2 center site
3 university
4 other, specify__________________________________

42. Rate the degree of involvement of respondent ☐ 50

1 highly involved and interested
2 moderately involved
3 neither involved nor uninvolved
4 moderately bored
5 bored, not interested

43. Comments on problems with interview, special circumstances

____

____

____

Attachment E - Publications, Newspaper articles; contains the following copyrighted material not available for ERIC reproduction.

"Preschool Programs and Later School Competence of Children from Low-Income Families," by Richard B. Darlington, Jacqueline M. Royce, Ann Stanton Snipper, Harry W. Murray, and Irving Lazar. Science; v208 pp 202-204, 11 April 1980. Copyright 1980 by the American Association for the Advancement of Science.

Parents as Partners: Preschool Learning and School Achievement

Irving Lazar
Cornell University

Perhaps the oldest argument in the history of American education concerns the malleability of intellectual ability. This argument has been particularly concerned with whether those who do poorly in school can significantly improve their performance with the help of some form of intervention.

Today we label the opposing sides in this debate as "genetic" versus "environmental"; during colonial days it was the "damned" versus the "chosen". At the heart of this continuing controversy is the conflict between the elitist and the populist traditions of our country. Beginning in 1954, with Brown vs. the Board of Education, federal policy has gradually moved toward the populist position. Poor performance in school is seen as the result of the child's social, ethnic, economic or physical condition, and changes in his environment, coupled with an enriched education, should, in this view, enable most, if not all children to perform competently in our schools and colleges.

Arrayed against this policy are, in addition to those who are irrationally racist, three principal groups: a group who believe that intelligence is principally determined by the individual's heredity, a group who believe that only intrafamilial intervention - usually maternal - at very early ages can have any significant influence on learning, and a group who believe that only macropolitical and economic changes can influence individual intellectual development. Jensen and Shockley are authors frequently associated with the hereditarian view; White champions the infant-maternal view, and Ogbu and Jencks represent the third of these positions.

Early intervention programs for children from low income families were built upon the premise that appropriate services from outside the family could compensate for the disadvantages which, it was believed, were responsible for the generally poor performance of these youngsters when they reached school. These programs always included an educational focus, and some had other services as well. Head Start for example, includes health services, and some Head Start grantees also provide psychological and/or social services. The question as to which of these views is valid is one of considerable policy concern. Head Start, in 1970, had an annual budget that exceeded 300 million dollars. If the critics of public intervention were correct in their premises, that money was being wasted. The Nixon administration, eager to cut expenditures for the poor, publicized an evaluation of Head Start that was designed by government employees and contracted to a private firm. The agency which designed the study had lost its bid to control Head Start, and, in fairness to the contractors, it should be said that they simply carried out their instructions.

I needn't point out to this audience the problems of the Westinghouse-Ohio University evaluation of Head Start. There was no random assignment of a control group, no baseline measures, no data on or control of the content of the programs studied nor any control for the length of intervention. Further, Head Start was not designed to change IQ. It was intended to help children do better in school. The correlation between IQ and school performance is about .40 - hardly enough to crow about, much less use as much of a predictor. Yet the Westinghouse-Ohio report that the difference between the IQ's of Head Start graduates and their classmates was insignificant at the end of the first grade was used as justification to try to close down Head Start. While the Nixon administration failed in that effort, they did freeze the Head Start budget at its 1970 level - and it stayed at that level throughout the Nixon and Ford years.

Now, how could one evaluate an early intervention program such as Head Start? Obviously, if a program is intended to enable children to perform at their age/grade level in ordinary public schools, then the legitimate measure of program success is the performance of those children in school as compared to an originally randomly selected control group for whom the same baseline measures are available.

One cannot conduct that kind of evaluation with Head Start itself. Its structure and operations have made it impossible to meet the requirements of a decent design. But there were alternatives. Head Start itself was inspired by a clutch of independent experiments in early intervention conducted at a number of research institutions in the late 1950's and early 1960's. It occured to me in early 1975 that if a sufficient number of their original experimental and control subjects could be found, that each of those studies could provide an independent test of the hypothesis that early education could affect later school performance. At least ten years had passed since the original experiments were concluded, and so the actual school performance could be ascertained without resort to predictions from test scores. With help from the Office of Child Development Ruth Hubbell and I identified fifteen of the early experiments that were large enough to survive heavy attrition. We thought that only a third of the original subjects could be located after a decade. As it happened, the investigators were able to locate, on the average, three quarters of the original experimental and control subjects, and comparisons of the baseline data of those who were located with those who were not found demonstrate no important attrition effects.

Fortunately, all but one of the original investigators agreed to give us copies of their original data for re-analysis, agreed to a common format for a follow-up of their subjects, and agreed to provide the follow-up data to us for pooled and cross-study analysis. This kind of collaboration is always difficult in our competitive society, but it worked here principally, I believe because all of the investigators are senior investiagtors and established leaders in child development. They have all reached the point in their careers where they could set aside the self-serving needs that so often doom cooperative efforts for a larger social purpose.

With barebones budgets, with significant help from their own institutions, and with enormous ingenuity and hard work, the investigators found their subjects, had less than two percent refuse to participate, and enabled us to demonstrate quantitatively that if you provide a high quality education to even the poorest of children that they will learn and that the earlier that education begins the longer will its effects be obvious. As the readers of this journal already know, our very conservative analysis of these findings demonstrated that the children in these programs are more successful in school than are their control groups. Specifically, program children were far less likely to have been assigned to special education classes or to be retained in one or more grades than were the control-group children. Among the older subjects, the program children were less likely to drop out of school before graduation from high school than were the controls.

The differences in assignment to special classes are particularly significant for school administrators. These differences held up even after controlling for pre- and post-intervention IQ and for a variety of background and familial variables. There can be little doubt that the differences were due to the effects of the preschool programs. These effects were so marked as to be cost effective. In one of the studies, the Ypsilanti-Perry Preschool Program, a formal cost-benefit analysis was carried out by a pair of economists. They demonstrated that the savings to the school district from reduced need for special education more than paid for the costs of the preschool program.[1] Various individual studies uncovered even finer-grained differences. For example, Susan Gray found that female program participants who became pregnant during their high school years were more likely to return to school and graduate after having their babies than were the control subjects who became pregnant during their high school years.

Altogether these studies included twenty seven different "treatments" or curricula. Several of the studies were originally designed to compare the effectiveness of various curricula. On these outcome criteria we found no differences between curricula. They all worked. This finding alone should be of particular interest to educators. Education has long been hunting for the best curriculum. These findings suggest that this is a futile, or at least, a commercial search. Any reasonably designed, age-appropriate set of learning goals can be achieved in a number of ways. There are many roads to Allah. Similarly, we found that while inservice training of teachers was positively related to successful outcomes for children, the prior training of the teacher was not. Professionals did well, as did paraprofessionals, as did parents serving as teachers of their own children. Nor did the site make a difference. Home-based, center-based and combination programs did equally well. Children from different family structures did well as did rural and urban children, black and white children, only children, youngest, oldest and middle children.

We found a cluster of five interrelated program characteristics related to positive outcomes:

Age of intervention - the earlier the better
Adult-Child ratio - the fewer children the better
Number of Home visits - the more the better
Direct participation of parents - the more the better
Services for families, not just the child, the better

These studies do not let us take this cluster apart, but current work in an experiment at 48 sites, conducted by the N.Y. State Department of Education, has demonstrated clearly that direct parent participation in preschool is related to academic achievement in the elementary grades. Their subjects aren't old enough yet to see how long that relationships will hold. The current follow-up of the oldest subjects in the Consortium studies is nearing completion and will compare various indices of life success in early adulthood of the original experimental and control subjects. It is clear already that those youngsters who participated in infant and preschool educational programs are doing better than their controls.

What do these findings mean for educational theory and educational policy? Obviously it means that early education, whether in or out of the home, whether by a teacher or a parent, can enable poor minority children to meet the requirements of public schools. Beyond that these studies suggest that the search for "the best" curriculum is like looking for a perfect Procrustean bed. They suggest, further, that, given good inservice training and supervision, that paraprofessionals can do a fine job - and so can parents. Indeed, it's my own belief that the basic reason that these early programs had such long-lasting effects is not curricular, but rather is a result of changes in the parents' values and anticipations for their children. A single shot of preschool seems hardly enough to produce a life-long change. I believe that the increased participation of parents and their success provided the value change that led them to encourage and reward their children's learning activities. As the professionalization of education drove parents out of their children's learning an essential condition for learning may have been severely damaged. Perhaps that loss can be prevented in the future as we learn to bring parents into partnership in the educational enterprise.

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The Consortium for Longitudinal Studies:
A Follow-up of Participants in Early Childhood Programs

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The Consortium for Longitudinal Studies is a cooperative effort of over a dozen investigators endeavoring to assess the long-term effects of early education on the lives of children from low-income families. The principal investigators, who conducted 14 longitudinal research and demonstration projects in the 1960s, agreed to pool their original data and to collect common follow-up measures for joint analysis in 1976 and 1979. This chapter describes the history of the Consortium, the design of the collaborative study, the characteristics of the data base, and the results of analyses performed thus far.

Background of Consortium

The Consortium for Longitudinal Studies was formed in 1975. A group of early childhood investigators who had independently conducted experimental infant and preschool programs for low-income children in the 1960s began a series of meetings chaired by Irving Lazar of Cornell University. The group, originally named the Consortium for Developmental Continuity, included: E. Kuno Beller, Temple University; Martin and Cynthia Deutsch, New York University; Ira Gordon (deceased, September, 1978); Susan Gray, George Peabody College; Merle Karnes, University of Illinois; Phyllis Levenstein, Verbal Interaction Project; Louise Miller, University of Louisville; Francis Palmer, Merrill-Palmer Institute; David Weikart, High/Scope Foundation; Myron Woolman, Institute for Educational Research; and Edward Zigler, Yale University.

Several concerns led to the formation of the Consortium. Since the release of the Westinghouse Report on Project Head Start in 1969, most people assumed that preschool effects lasted little past the end of the program. The Consortium members felt that their subjects (aged 8 to 18 at that time) were old enough to provide a test of the long-term effects of preschool.

The meetings resulted in a decision to pool the data which each investigator had collected individually and to collaborate on a common follow-up data collection and analysis effort. Each investigator agreed to send his or her raw original and common follow-up data to Cornell to form a Consortium data bank. Parallel or pooled analyses with raw data would be undertaken at Cornell, but each member retained full rights to release raw data to others and to publish his or her own findings. Richard Darlington of Cornell joined the Consortium in 1977 to direct Consortium data collection and analyses along with Irving Lazar. Robert Jester of the University of Florida, now a principal investigator for the Gordon project, became a Consortium member after Ira Gordon's death. The Consortium for Longitudinal Studies originated and continues to exist principally through a strong commitment to a collaborative effort.

Description of Consortium Early Child Development Studies

The early education programs in the Consortium studies were located in urban and rural sites in the Northeast, Southeast, and Midwest (see Table 1). The programs were active from 1962 to 1972; only Levenstein's Verbal Interaction Project, Weikart's Perry Preschool Program, and

Karnes' GOAL program are in operation at the present time. Depending on the theoretical interest of the investigator, curricula included programs based on the Bank Street child development model, on Montessori methods, on Piagetian theory, on the Bereiter-Engelmann method and others.*

The Consortium studies can be categorized as center-based, home-based, or combination home visits/center programs. Center-based studies typically provided nursery school programs for 3- or 4-year-olds with varying degrees of structure in the program curriculum. Instruction generally took place in small groups but in some cases was on a one-to-one basis.

Home-based studies directed their educational efforts primarily toward the parent, usually the mother, as the major instrument of change and influence in the child's life. Activities, toys, and games were brought to the family home by a parent educator or home visitor, who taught the mother to use the activities and to promote her child's development through parent-child interaction. These programs served infants and toddlers.

The third group of studies combined these approaches, providing a center-based nursery school program along with a periodic home visit which involved both parent and child. In some programs the emphasis during the home visit was closely related to the center programs; in others, it had a more general content.

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* For full description of Consortium programs, see Consortium, 1977; Consortium, 1978; Lazar, et al. (in press) and individual project reports listed in the reference list.

Description of the Sample

Since all projects were originally conducted independently, each had different sample selection criteria and procedures for assignment to program or control groups. Table 2 summarizes sample selection criteria. The criteria were similar for most projects, since projects generally selected children from low-income families. Selection criteria differentiated among projects as follows. The Palmer project selected half lower-class and half middle-class black males. Deutsch, Gray-ETP, Palmer, and Weikart-Perry selected only black subjects. Karnes specified a minimum IQ of 70, while the Weikart-Perry and Curriculum Demonstration projects specified a maximum IQ score of 84. All projects except the three in the New York City area were open to non-English-speaking children, but only the Woolman project actually served a significant number of such children. As seen in Table 3, most of these criteria were reflected in the demographic characteristics of the original samples.

The projects also differed in the manner in which they assigned children to program or control groups. The importance of adequate procedures for assignment to program or control cannot be overemphasized. Therefore, the sample selection procedures for each project were reviewed and classified (Consortium, 1977). Seven projects were classified as experimental -- Deutsch, Gordon, both Gray projects

Palmer, Weikart-Perry, and Weikart-Carnegie. Because they were originally designed to answer questions other than a simple program/control comparison, five were classified as quasi-experimental for our purposes -- Beller, Levenstein, Miller, Woolman, and Zigler.* Two projects were concerned solely with curricula comparisons and had no control groups -- Karnes and Weikart-Curriculum Demonstration.

The Consortium data base is unusual: it has both cross-sectional and longitudinal dimensions; furthermore, because it is composed of a number of research and development projects, it constitutes a data base permitting meta-analysis. Its complexities become evident as soon as one attempts to "describe the sample." Due to attrition (both in the form of inability to locate subjects and of decisions not to follow certain groups or projects because of design or monetary constraints) numerous "samples" could be delineated. Two such "samples" are described here: the original sample (on whom initial data was collected) and the 1978 follow-up sample (on whom any of the common Consortium instruments were collected).

The original sample consisted of 3,177 children from 13 projects (Table 3). For the median project, 67.3% of the children were enrolled in a preschool program, 47.4% of the children were female, and 90% were black. At the time of entry into the program, the medians on background variables were: mother's education, 10.3 years; Hollingshead SES score* of 64; three siblings; and Stanford-Binet pretest IQ score of 90.8. Individual project differences are minor, except where accounted for by sample selection criteria mentioned previously.

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* Beller, for example, compared three groups on age of entry to schooling (nursery, kindergarten, and first grade). Miller's project had an experimental design for comparing different curriculum groups, but a quasi-experimental design for comparing program to control.

The 1976 follow-up sample was composed of 2,008 children from 11 projects (Table 4). The median project had 64.9% children enrolled in the program, 94.8% of them were black, and 50.0% were female. The medians on the follow-up sample for background variables measured at the time of entry into the projects were: mother's education of 10.4 years; SES (Hollingshead ISP) of 64.0; 3.2 siblings; and pretest Stanford-Binet IQ score of 92.3. Thus, on a macro level, the 1976 follow-up sample was very similar to the original sample despite the attrition and changes in groups and projects included in the follow-up.

The differences between the original and 1976 follow-up samples can be traced to several sources: (a) the exclusion of certain projects from the follow-up (Gray-FOHV, Weikart-Curriculum Demonstration, Weikart-Carnegie); (b) the exclusion of certain groups or waves in a project from the follow-up (e.g., Deutsch groups three through eight); (c) the addition to the follow-up sample of one project (Woolman) that was not contained in the original data base; and (d) the attrition of individuals within the projects which were followed-up.

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* The Hollingshead Two-Factor Index of Social Position (ISP) is based on head of household education and occupation. The scale ranges from 11 (representing highest social position) to 77 (representing lowest social position).

The samples on which we have data are shown in Tables 3 and 4; the samples used for analyses were, of course, subsets of the total samples. Analysis samples could differ from total samples in three ways.

1. Whole projects could be eliminated from the analysis because (a) they did not collect the necessary dependent variable for the analysis (e.g., Beller did not collect WISC scores); (b) the design of the project was inappropriate for the analysis (e.g., the Karnes and Weikart-Curriculum Demonstration projects had no control groups and, hence, were eliminated from the program-control comparisons); or (c) attrition analyses indicated problems which endangered validity (this occurred only for the Deutsch project).

2. Missing data could eliminate some subjects within a project which was included in the analysis.

3. In some cases, projects had two or more comparison groups. In order to make the comparison as closely experimental as possible, we eliminated the control group which we judged to be less valid for program/control analyses (e.g., "after-only" subjects selected in first grade). This procedure reduced the sample size, and, hence, the power. Beller, Gray-ETP, and Levenstein each had one control group eliminated from analyses for this reason.*

Design

The Consortium research is a four-wave panel study: (a) the initial data collected independently by the investigators prior to the formation of the Consortium; (b) postprogram follow-ups collected independently by the investigators before 1976; (c) the 1976 collaborative Consortium follow-up; and (d) the 1979-1980 collaborative Consortium follow-up now in progress.

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* The Consortium data base does contain these groups, however, and they are included in Tables 3 and 4.

Wave 1 data were collected when subjects entered the various preschool programs, over the years 1962-1972. Each project had its own research design, generally comparing one program group or a number of program groups to a control group, or comparing among a number of program groups. Except for cross-fertilization through professional communications, each investigator independently planned and implemented his or her own research design and data collection effort. Later programs were influenced by earlier programs in some cases. Wave 1 data were obtained primarily from the intake or subject-selection interviews that each investigator had designed for his/her own purposes at that time. The data included primarily demographic and family background data and preprogram scores on a variety of measures, generally including an intelligence test.

Wave 2 data include the early follow-ups conducted independently by each investigator prior to 1976. Depending on research design and availability of funding, investigators conducted an immediate post-program assessment and a number of follow-up assessments until subjects were 8, 9, or 10 years old. The number of annual follow-ups ranged from two to seven.

Waves 1 and 2 raw data were sent to Cornell in early 1976 to form the Consortium data bank. Data sent on computer cards or raw data sheets were checked for accuracy and internal consistency. Cross-tabulations of type of data by project site revealed variables which were collected by enough investigators to permit common analyses. After consultation with each project site, IQ test scores and selected demographic data were recoded into a common format and placed on computer file.

Wave 3 data were collected collaboratively in 1976-1977 when the sample ranged in age from 9 to 19 years old. The Wave 3 data included the 1976 Consortium Parent and Youth Interviews, the Wechsler Intelligence Scales (Revised), achievement test scores primarily recorded from school records, and school career information recorded by field workers on Consortium School Record Forms.

Some investigators had maintained contact with their sample over the years through activities such as home visits, Christmas cards, and post office forwarding addresses. Other investigators, however, who had not attempted to reach their sample for up to 10 years, experienced difficulty locating subjects and school records. They examined name and address listings in local schools or Boards of Education. Because of school reorganization or other factors, some investigators were unable to find subjects on school listings and had to rely on other sources, including retired school teachers living in the community, former high school athletes, relatives, neighbors, and subjects already interviewed.

In order to assure consistency of data collection across sites, the group at Cornell conducted training sessions for field site supervisors and provided video tapes to demonstrate interviewing techniques. Most items were precoded, and all instruments were printed on two-part carbonless paper; field sites retained one copy and mailed the original to Cornell where office coding, editing, and keypunching were done. Each project site received a duplicate set of their data on computer cards.

The Wave 4 follow-up of 1979-1980 is currently underway. All participating investigators are collecting detailed information on subjects' school careers. The information is recorded on Consortium School Forms at each field site. Investigators with subjects over 18 years old are administering the 1979 Consortium Interview, either by telephone or in person. Some project sites have collected achievement test scores, either individually-administered or collected from school records. As in the 1976 follow-up, Cornell staff coordinate data collection at the field sites, primarily by telephone, and then office code, edit, and keypunch the data.

Measures

A wide variety of status and process variables were collected by individual investigators over the four waves of data collection. In the first two data collection waves before the formation of the Consortium (1962 to 1975), investigators collected diverse preprogram child and family measures (Wave 1) and then numerous and varied postprogram outcome measures over several years (Wave 2). In the Consortium follow-ups of 1976 and 1979 (Waves 3 and 4), a core of common measures was collected. The measures included in the Consortium data bank are described below.

For Wave 1, the independently-collected initial data, all investigators, except one,* collected preprogram background data such as education and occupation of mother and father, number of siblings, and father presence in the home as well as sex and ethnicity of child, mother's age at childbirth, and other variables. Adding certain variables to the Consortium file was problematic. Variables such as father presence and number of siblings, for example, did not have a standard definition across projects. In some cases, data from a project for a certain variable could not be transformed into a common Consortium format, but usually variables were retained despite some problems in comparability.

In addition to Wave 1 demographic variables, most projects collected a battery of pretests such as Cattel, Stanford-Binet, Peabody Picture Vocabulary Test (PPVT), Bayley, and a variety of other standardized or project-designed measures. Of these measures, only the Stanford-Binet and PPVT pretests were collected by a sufficient number of projects to justify inclusion in the common data base.

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* The exception was Beller who collected background demographic data retrospectively as part of the 1976 Consortium Parent Interview.

The second wave of testing, also conducted independently by the investigators, consisted primarily of standardized IQ and achievement tests and project-developed tests. The majority of projects collected Stanford-Binet and PPVT IQ scores at the various Wave 2 follow-up testing periods. The number of children taking the Stanford-Binet by age for each project is shown in Table 5. All Binet and PPVT scores are included in the Consortium data file. Other tests were collected by several investigators but not included in the Consortium data base. For example, standardized tests collected by more than one project were: ITPA (4 projects); WISC (4 projects); Leiter (2 projects); Bayley (2 projects); Preschool Inventory (2 projects); California (4 projects); Metropolitan (5 projects) and Wide Range (2 projects) achievement tests, and Metropolitan Readiness Test (3 projects). Individual investigators also collected a large number of tests that were not replicated in any other project (e.g., socio-emotional and perceptual measures).

The decisions on the choice of common instruments to be used in the Wave 3 follow-up study of 1976 evolved from lengthy discussion of alternatives by the Consortium members over a 6 month period. Because of financial restrictions and the high cost of locating subjects who had not been interviewed or tested for many years, it was necessary to choose a limited battery of measurements. The Consortium members agreed to administer the Consortium-developed Parent and Youth Interviews and appropriate Weschler Intelligence Scale, and to obtain data from school records and achievement tests administered by the schools.*

The 1976 Consortium Parent Interview was initially developed from interviews used by individual investigators. It was designed to obtain comprehensive information on household composition; family members; housing and mobility data; parental attitudes toward, aspirations for, and evaluations of their child; information on the child's medical history; the parent's current relationship with the child; and parents' assessment of the preschool program.

The 1976 Youth Interview also drew upon interviews developed previously by the principal investigators. It included information on the child's status in school; his or her educational and occupational aspirations; leisure time activities and interests; employment status; and integration into his or her peer group, family, and the larger community.

The 1976 Consortium School Record Form provided data on the subjects' current educational status (graduated from high school, dropped out, currently in school); whether subject was ever placed in special education classes (and type of class); how often he or she was retained in grade or accdelerated; and the code number and type of school attended for 4 specified years.

Diverse achievement tests were administered by the many school systems including California Achievement Test, Comprehensive Test of Basic Skills,

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* In addition, four projects provided detailed school information (e.g., course marks, tracking, attendance) that they recorded on Consortium Long School Record Forms.

Iowa Test of Basic Skills, and Metropolitan Achievement Test. Depending on the information recorded in school records, field workers recorded raw scores, grade equivalent scores, or standard scores on 1976 Consortium Achievement Test Forms for up to four different academic testing periods.

For each project, the number of cases with Wave 3 follow-up data by instrument is shown in Table 6. The number of cases in the 1976 follow-up with data on at least one instrument was 2,008; the last column on the right indicates the number of cases in the follow-up for each project.

The Wave 4 data collection of 1979-1980 utilizes two Consortium-designed instruments: the Consortium 1979 Youth Interview and the Consortium 1979 School Data Form. Six sites have interviewed or will interview their subjects (Beller, Deutsch, Gray, Karnes, Miller, and Weikart). For some sites, the 1979 Consortium Interview forms part of a longer interview designed by individual investigators. The Consortium Interview focuses on determining subjects' status in the areas of education, employment, and family formation. Additional items include job satisfaction, attitudes toward work, educational and vocational aspirations and expectations, sources of income, desired and expected family size, stresses and supports during childhood, self-evaluation, self-esteem, and life priorities.

The 1979 School Data Form consists of two sections: (a) information for each academic year including attendance, special class placements, grade retentions, and participation in supplemental tutoring or enrichment programs; honors; course marks for 4 years; class rank; suspensions, expulsions, and dropout history and reasons; and (b) detailed information on "problem" children, including diagnoses and reasons for referral to special services and critical home and school events.

As of 1979, measures included in the Consortium data base are: demographic variables and Stanford-Binet and PPVT pretests for Wave 1; Stanford-Binet and PPVT post-program IQ scores for Wave 2; and all Consortium instruments for Wave 3 follow-up (i.e., 1976 Parent and Youth Interviews, achievement tests for 4 years; 1976 WISC scores, and school record information). We expect to add the Wave 4 data to the Consortium file in early 1980 (i.e., 1979-80 Consortium interviews, school data, and achievement scores).

Methods for Analysis

The most fundamental methodological decision on data analysis concerned the conceptualization of the data base. One alternative was to view the data base as one large quasi-experiment; the other was to conceptualize the data as a series of independent replications of quasi-experiments. The choice of conceptualization has far-reaching implications for methodology. The first alternative implied that the data should be pooled and a secondary analysis performed. The second implied that within-project analyses should be performed and that only the results should be pooled. We refer to the latter approach as multi-sample secondary analysis.

Conceptualizing the data as one large quasi-experiment and pooling the data would greatly reduce the number of analyses necessary to answer any particular question. However, there were a number of disadvantages associated with this option. First of all, it would be difficult to account for all of the differences among projects (e.g., design, geographic, cultural, and cohort differences) in a joint analysis. Some of these differences could be estimated and entered into equations as independent variables. Others were essentially unmeasurable and could only be accounted for by entering a string of dummy variables representing each project. Secondly, projects had different proportions of program to control children and could thus yield artifactual results. As a simple example, consider an analysis which includes two hypothetical programs which were both ineffective. Let program A have a treatment group of size 100 and a control group of size 20, each with mean pre- and post-test IQ scores of 90. Let program B have 20 treatments and 100 controls, with pre- and post-test means of 80. Analyses of each group separately would correctly show no treatment effect. If the two samples were pooled, however, the resulting sample would show a post-test mean difference of 6.7 points. Thirdly, a number of variables had different meanings across projects. For example, the meaning of assignment to special education could vary depending on school district policy. Demographic variables such as father presence were also specified differently in different projects. The effect of such variables could be assessed reliably only within a project. And last of all, a joint data analysis using a number of independent variables would have severe missing data problems. For almost any independent variable of interest, at least one or two projects did not collect the variable.

We chose to utilize the second conceptualization of the data base. Viewing the data base as a series of independent replications and analyzing each project separately was an historically more accurate conceptualization since the projects were essentially independent research and demonstration enterprises designed to test the effectiveness of a type, or types, of preschool. Calculations of statistical power indicate that this conceptualization reduces power somewhat, but not to a great extent. Moreover, analyzing each project independently would force the analyst to concentrate on the design characteristics of each project. This conceptualization also provided greater flexibility in dealing with missing data and with "equating error" as explained below. The most serious drawback of this conceptualization was the possibility of small cell sizes within a project when controlling for several independent variables.

Given the decision to use multi-sample secondary analysis, the following general methods were applied. Parallel analyses were performed separately for each project, e.g., hypothesis tests comparing program children to control children from the same project. The $\underline{p}$ values for the variable(s) of interest were pooled across projects using an appropriate basis of comparison, which could be, for example, age, grade, or number of years after the project. For instance, in the IQ analyses, if the basis of comparison were age, all analyses for a given age would be pooled regardless of the number of years after the program. On the other hand, if the basis of comparison were number of years after the project, all analyses for a given number of years after the project would be pooled, regardless of the age of the children in each analysis.

Two techniques were used to pool p values, one for directional results, the other for nondirectional results. For directional results (e.g., questions of preschool effectiveness vis-a-vis controls) p values were converted to z scores, which were summed and divided by the square root of the number of projects. The resultant pooled-z score was then converted to a p value. Since it takes account of both positive and negative results, this pooled significance level estimated the "average" effect of preschool across projects. For nondirectional results (e.g., interaction effects or the effect of a set of variables), the individual p values were converted to z^2 scores, that were summed and compared to a chi-square table with degrees of freedom equal to the number of projects (Mosteller & Bush, 1954).

An important concern in pooling results is the robustness of the hypothesis test where "robustness" refers to the ability of an overall significant result to withstand the removal of the most strongly significant sample. There were presumably known or unknown design problems in at least one or two projects and we don't know which one or two. To take this possibility into account, we deleted the program with the strongest result from the pooled-z analysis and recalculated the pooled-z score. If a significant result evaporated, the finding was not considered a strong one.

In looking at results for individual projects within the context of the data base as a whole, the Bonferroni/Ryan statistical technique was used to correct for multiple comparisons. Because of the large number of tests of hypotheses, it would be possible to obtain "significant" results by chance alone. The Bonferroni/Ryan technique adjusts for multiple comparisons by multiplying the significance level of each result by the number of significance tests performed (Darlington, 1978).

The technique of performing only within-project analyses and pooling results gave us a good deal of flexibility in dealing with a serious problem of both longitudinal and cross-sectional studies: equating error (Hilton & Patrick, 1970).

Equating error means that one can validly compare neither (a) scores from different tests even if they were designed to measure the same trait (e.g., intelligence) nor (b) scores from the same test taken at different ages. Either type of comparison would be precarious due to differences in content and standardization. The contents of two different tests obviously differ and it is not known precisely to what extent this content difference might bias comparisons. Moreover, the "same" test differs greatly in content at different ages. Obviously a Stanford-Binet IQ test for age 3 has a vastly different content from a Stanford-Binet IQ test for age 15. Even if there were no problems in the comparability of content, the standardization procedures of even the major IQ and achievement tests are not adequate to ensure comparability either between tests or for the same test at different ages. The tests are standardized cross-sectionally rather than longitudinally; further, the standardization is performed on a relatively small sample -- for the Stanford-Binet on 200 or fewer children per age group. Even when "smoothed," the 1972 restandardization of the Binet revealed differences as large as ten points between the old and new standardizations. Thomdike explained this as reflective of cultural change, but a feasible alternative hypothesis would be that the difference is due to sampling error.

To avoid the pitfalls of equating error, our analyses evaluated the scores of program children only in relation to the scores of a group of control children on the same test at the same age. Pretest scores of the program children were never compared to posttest scores to determine an IQ "gain" score. The program is said to have increased IQ scores only if it increased scores relative to a control group.

Since analyses are performed only within projects, we could reliably perform analyses that, due to equating error, could not be done on data pooled in one large quasi-experiment. For instance, for analyses 3 years after the program, children of project A (which terminated at age 2) would be 5 years old and children of project B (which terminated at age 5) would be 8 years old. We could not validly pool data from the two projects because this would compare age 5 scores with age 8 scores. However, by analyzing project A at age 5 and project B at age 8 and pooling the significance levels using the pooled-z technique, we have a combined significance level for 3 years after the program without comparing ages 5 and 8 scores. Similarly, the data from two projects with different intelligence tests could not be pooled validly because of biases in the standardizations. However, the two projects can be analyzed separately and the significance levels pooled. The key to this approach is that within-project analyses are performed only for the same measure at the same age. The results can then be combined with those of other projects even if those projects used a different measure of the same phenomenon (e.g., different achievement tests) or administered the test at a different age, providing there is a logical basis for comparison; e.g., number of years after the program.

Another problem encountered in longitudinal studies is test-retest bias. In terms of internal validity this bias is largely controlled by comparing program and control groups which have been tested the same number of times. In terms of the external validity of test-retest bias, results from the Consortium study should be applicable to any group of children who have been tested repeatedly -- which includes most American school children.

An additional problem of longitudinal research is what Campbell (1971) and others have called "temporal erosion." While a developmental psychologist might quarrel with the simplicity of Campbell's statement that "all relationships weaken with time," it would certainly be expected that many, if not most, program/control differences would erode with time. The lag between the end of the subjects' preschool experience and the time of 1976 follow-up data collection is as much as 14 years -- time enough for even very gradual erosion to take a heavy toll. On the other hand, it is possible that Consortium programs not only taught concrete skills, but altered the context in which the child operated. For example, the mother-child or teacher-child interactive system may have been affected by the program, setting in motion a new interactive system that operated to minimize the erosion of the program effect.

One of the most serious threats to any longitudinal study is the problem of attrition. Over time, subjects drop out of a study due to moving, death, or a myriad of other reasons. We conducted extensive attrition analyses for each project aimed at answering four questions.

1. Are there different rates of attrition for program and control groups? (Tested by a 2 X 2 crosstabulation of program status and dropout status.)

2. Do the final samples differ on some important characteristic from the dropouts? (Tested by examining the dropout main effect in 2 X 2 ANOVA's with program status and dropout status as factors and mother's education, SES, and pretest IQ as dependent variables.)

3. Are there any instances of differential attrition? That is, are different kinds of children selectively retrieved in program versus control groups? (Tested by the program-dropout interaction vector in the 2 X 2 ANOVA's described in question 2.)

4. Do final program samples and final control samples differ on some important characteristics? (Tested by a t-test between program and control groups for the final sample.)

The first three of these questions are questions about attrition in the strict sense of the term. That is, they examine the extent to which final sample treatment and control groups represent the original sample groups. While question 4 is not, strictly speaking, a question of attrition, it complements the attrition analysis by answering the simpler question of whether there were significant differences on the three dependent variables in the final sample itself.

The Consortium 1976 follow-up involved the collection of four types of information, or instruments: individual intelligence tests, interviews of children and mothers, and school records. Attrition was defined as the failure to report information for a particular child on a particular instrument. This definition was applied because the different methods for collecting information occasionally resulted in considerable divergence among the samples receiving different instruments. For example, collection of Youth Interview and WISC-R data required actual contact with the child, while collection of the School Record Form data required, instead, contact with the child's school records, which could be either more or less difficult than actual contact with the child depending on the cooperation and oragnization of the school district.

Our analyses did reveal some rather severe problems concerning attrition in the Deutsch data. As a result, the Deutsch data were omitted from all our major analyses. In data from the remaining projects, our analysis supported the view that the final samples were representative of the original sample and the final sample treatment and control groups appeared to be equivalent. The most complete report on our attrition analysis is contained in Consortium (1978).

Response rates were computed from data recorded by field workers. For every subject who was in the earlier samples (Waves 1 and 2) and for every instrument, projects specified the disposition of each case with the reason that testing or interviewing was not accomplished. The categories of noncompletion included: refusals (either to be tested or to permit examination of school records), subject was located but unable to be tested (e.g., living out of area), current location unknown, or subject was "out of sample" (e.g., in group not scheduled for 1976 testing).

Virtually all of the attrition was due to simple inability to locate subjects or other sources of data, such as school records. Less than 3% of our subjects or parents refused to participate.

Outcomes Studied

We have examined four main outcomes in our study of the effect of Consortium childhood programs: school competence, measured as special education placements and retention in grade; intelligence test scores, measured at several time periods; standardized achievement test scores; and certain non-cognitive outcomes, particularly achievement-orientation.

Improved school competence of children from low-income families was one goal of the Consortium infant and preschool programs, just as it is for Head Start programs nationwide. Progressing through school with agemates instead of being assigned to special education classes or retained in grade is a direct measure of school competence that is visible and ecologically valid. The child who requires assignment to special education classes or grade retention has not met the requirements of his or her school. Success or failure in the elementary and secondary school years is an important variable in the status attainment process. It has critical implications for an individual's ultimate educational, occupational, and economic attainment. Furthermore, the labeling and ability streaming (tracking) associated with school failure have important developmental implications.

The second outcome measure we studied was intelligence. Intelligence tests have been a focus of controversy ever since they were devised as a method of quantifying the nebulous concept of intelligence. They have been accused of being culturally biased, of reflecting "achievement" and motivational variables as well as cognitive ability, and of being used for social control. Empirically, however, intelligence test scores remain the best available predictor of school performance and they are commonly used in educational evaluations.* It is only logical to expect a correlation between IQ scores and school performance since scoring well on an intelligence test and performing well in school both require similar behaviors, including spontaneous verbalization, persistence at a task, ability to follow instructions, and ability to adapt to structured surroundings. Thus, intelligence test scores are used in this study as an indicator of expected performance in schools as they currently exist in our society.

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* Indeed, if IQ tests are culturally biased, so are most school systems.

Achievement tests administered by the schools were also used as outcome measures. Scores on standardized achievement tests provide information on pupil learning that is designed to be independent of local curricula and individual teachers. Like intelligence tests, achievement tests have been a subject of controversy -- whether they are primarily measures of ability rather than learning, the utility of norm-referenced versus criterion referenced tests, etc. Nevertheless, we utilized achievement tests as the most comparable measure of children's mastery of specific academic content across projects.

A number of non-cognitive outcomes (achievement-orientation, maternal and child aspirations, and child's self-assessment of school performance) were investigated using questions on the 1976 parent and child interviews. We have regarded the analyses of noncognitive outcomes as exploratory because of the difficulties in measuring attitudes and values.

Of the outcomes considered thus far, only intelligence test scores were recorded at several points in time and are feasible to analyze over time.* The Wave 4 data collection of 1979-80 will provide repeated measures of placement in special education and retention for each year. We expect these analyses to be methodologically challenging.

Results

Numerous analyses have been performed, both by the Consortium staff using the Consortium data base and by the individual projects using their own data. We report here the results from analyses on the Consortium data base, which have focused on two main questions thus far:

1. Does participation in an early childhood program have long-term positive effects on various outcome measures?

2. Do certain types of children benefit more than others from the preschool experience?

The main and differential effects of preschool on school competence and intelligence test scores will be summarized first. Then, the results of achievement test and the exploratory noncognitive analyses will be briefly summarized.

In the 1976 follow-up, investigators examined school records to determine whether their subjects were progressing with their agemates or had run into difficulty and were assigned to special education classes or retained in grade at any time in their school careers.* We combined the two indicators to form a composite variable: students who met school requirements versus children who failed to meet school requirements and were placed in special education classes or retained. Because schools have different policies, practices, and resources for students who fail, each Consortium program group was compared to its own control group within a school district. Results were tested for robustness by deleting the data set with the strongest finding.

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* Achievement test scores were also collected at several time periods, but missing data and noncomparability of the various tests would make analysis over time problematic.

As shown in Table 7, preschool graduates had significantly fewer school failure experiences than did the control groups. Across the seven projects with appropriate data, the median rate of failure to meet school requirements was 45% in the control group but only 24% in the program group. The results were statistically significant for the group of seven data sets ($p < .001$) and robust after deleting the strongest project. The results were even stronger for data sets with experimental research designs.

When the two indicators of school competence were analyzed separately, preschool apparently had a stronger effect on rate of special education assignment than it did on grade retention. As shown in Table 8, the median rate of assignment to special education classes was 29% for the control group but only 14% in the program group among the six data sets with appropriate data. The combined results were statistically significant ($\underline{p} < .001$) and robust. For grade retention (not shown), across the eight data sets with appropriate data, program children were significantly less likely to experience grade retention than controls ($\underline{p} = .018$), but this result was not robust. The median rate of grade retention was 25% in the program group compared to 31% in the controls.*

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* For the median project, subjects were in 7th grade when school data were collected. Special education placements included classrooms for children diagnosed as mentally retarded (educable or trainable), learning disabled, or emotionally disturbed. Speech and hearing problems were not classified with special education placements.

Although program and control groups generally did not differ significantly on background variables measured before preschool, we used standard multiple regression to control for observed differences in school competence analyses. Covariates controlled in this way were: level of maternal education (in grades completed), father presence versus absence, number of siblings, sex, ethnicity (black versus white), and preprogram IQ score. On the average, controlling for background and initial cognitive ability factors, 16% more of the preschool graduates than controls were on grade in regular classes rather than assigned to special education classes or retained in grade. Results were significant (pooled $p < .001$) and robust.

The second question we addressed was whether certain kinds of children benefitted from preschool (the differential effects of the school competence outcome measures). This question derives from a widely held assumption that, for example, children with higher initial IQ scores or those from relatively higher SES backgrounds benefit more from preschool programs. We used regression analysis to test for evidence of differential effectiveness. Preschool interaction variables were formed by multiplying the preschool categorical variable (program versus control) by each background factor. We found that the positive effects of preschool on school competence were not affected by the child's preprogram IQ score, gender, mother's educational level, family structure, family size, or ethnic group.*

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* The individual $\underline{p}$ values were: Gordon (.956), Gray (.291), Palmer (.006), Weikart (.065), Beller (.475), Levenstein (.531), Miller (.248, favoring controls), and Zigler (.452). For the four experimental projects, the results were stronger (pooled $\underline{p} = .004$), but again they were not robust.

The effect of preschool on IQ scores over time was also analyzed. Multiple regression analyses were performed to assess the effect of preschool on IQ score controlling for background variables (sex, pretest IQ, mother's education, number of siblings, and father presence) for each project at each age. The effect of preschool was estimated by the regression coefficient for the program-control status dummy variable. These results were pooled, according to the number of years after preschool, using the pooled _z_ technique. Overall results indicated that preschool had a significant effect on intelligence test scores for up to 3 or 4 years after the end of the preschool program (see Table 9). Similarly, the difference was significant for experimental projects only at immediate post-test, 1 year after and 2 years after. Thus, results indicate that preschool increased children's IQ scores for at least 2 years after the program with some evidence of an increase for 3 or 4 years afterwards. Analysis of the WISC-R data collected in 1976 indicated that the effect of preschool on IQ scores is not permanent.*

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* We repeated these analyses using logit analysis, a statistical technique similar to regression but designed specifically for dichotomous dependent variables. The results were essentially identical.

We also examined the possibility of differential effects of preschool on IQ scores (i.e., the possibility that preschool affected one type of child differently from another) by looking at the interaction vectors between the above mentioned background variables and program/control status. Results did not indicate any differential effects.

Analysis of achievement test data collected by the schools involved far greater methodological problems than any of our other analyses due to more missing data, children taking different tests or different versions or levels of a test, etc. The achievement test analyses are still ongoing. Initial pooled results indicate a significant difference favoring the program groups in mathematics subtests but not in reading, at the fourth grade (the grade for which most projects had data).

Analysis of non-cognitive outcomes revealed several program/control differences. Program mothers' aspirations for their children were higher than the children's aspirations for themselves, while this pattern was not evident for the controls. Program children were "proud of" achievement-related actions more often than control children.

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* Results were not pooled across projects because the tests were administered at different ages and at different number of years after the programs. Only the projects with the youngest samples had IQ score differences in 1976. (See Consortium, 1978)

We have begun to construct path analyses to explore the antecedents and consequences of school success and failure incorporating family background variables, preschool participation, and measures of subjects' intellectual ability.

Summary

The Consortium for Longitudinal Studies has provided evidence that early childhood programs can have an impact on the academic careers of low-income children. This effect can be measured both in terms of performance on standardized tests and in terms of actually meeting the requirements of normal progression as defined by the schools. The four waves of data collection continue to provide an opportunity to evaluate the effect of preschool on various outcomes as the sample reaches young adulthood. Further, the Consortium data can permit the testing of models which explore the interrelationship of variables over time. Research questions can be addressed in areas of cognitive and social development, family background and process effects, family formation, and educational and occupational attitudes and attainments.

Table 1

Characteristics of Early Education Programs and Ages of Subjects for Each Data Set

Principal Investigator	Early Education Program	Location	Type of Delivery System	Subject Birth Year	Age at Entry to Program	Length of Program (years)	Years of Program
Beller	The Philadelphia Project	Philadelphia, Pennsylvania	Center	1959	4 years	1 year	1963-64
Deutsch	Institute for Developmental Studies	New York, New York	Center	1958-61	4 years	5 years	1963-71
Gordon	The Parent Education Program	Gainesville, Florida	Home	1966-67	3 mos. to 2 years	3 years	1966-70
Gray	The Early Training Project (ETP)	Murfreesboro, Tennessee	Center/ Home	1958	3.8 or 4.8 years	14 mos. or 26 mos.	1962-65
Gray	The Family-Oriented Home Visitor Program	Nashville, Tennessee	Home	1971	birth to 1 year	1 or 2 years	1970-73
Karnes	Curriculum Comparison Study	Champaign-Urbana, Illinois	Center	1961-63	4 years	1 year	1965-66 (2 waves)
Levenstein	The Mother-Child Home Program	Glen Cove, Manhasset & Freeport, Long Island, New York	Home	1964-68	2 years & 3 years	1-1 1/2 years	1967-72
Miller	Experimental Variation of Head Start Curricula	Louisville, Kentucky	Center & Center/ Home	1964	4 years	1 year	1968-69

Table 1 (Cont.)

Principal Investigator	Early Education Program	Location	Type of Delivery System	Subject Birth Year	Age at Entry to Program	Length of Program (years)	Years of Program
Palmer	Harlem Training Project	New York, New York	Center	1964	2 or 3 years	1 or 2 years	1966-68
Weikart	Perry Preschool Project	Ypsilanti, Michigan	Center/ Home	1958-62	3 years (1st wave) 4 years	2 years (1st wave) 1 year	1962-67 (5 waves)
Weikart	Curriculum Demonstration Project	Ypsilanti, Michigan	Center/ Home	1964-65	3 years	2 years	1966-70
Weikart	Carnegie Infant Program	Ypsilanti, Michigan	Home	1968	3 mos. to 2 years	16 mos.	1967-70
Woolman	Micro-Social Learning System	Vineland, New Jersey	Center	1966-68	4 to 5 years	1 to 4 years	1969-73
Zigler	New Haven Follow- Through Study	New Haven, Connecticut	Center	1962-64	5 years	4 years	1967-71

Table 2

Sample Selection Criteria Used by Projects

Project	Age (in years)	Sex	Race	IQ Score	Language	SES
Beller	4,5,6					low*
Deutsch	4-8		Black		English	low
Gordon	birth,1,2					low
Gray-ETP	4,5		Black			low
Gray-FOHV	birth,1					low
Karnes	4			≥ 70		low*
Levenstein	2,3				English (early waves)	low
Miller	4					low
Palmer	2,3	Male	Black		English	1/2 low 1/2 middle
Weikart-Perry	3,4		Black	< 85		low
Weikart-CD	3			< 85		low
Weikart-Carnegie	birth,1					low
Woolman	4,5					low*
Zigler	5					low

* Indicates projects which did not have a formal specification of "low income." For other projects, low SES was specified on a measurable scale (e.g., Hollingshead ISP).

Table 3

Background Characteristics of the Original Sample

Project (n)	Birth Year	% in Program	% Black	% Female	Mean Mother's Education	Mean SES	Mean Binet Pretest IQ	Mean No. Siblings
Beller (170)	1959	34.7	90.0	47.1	10.5*	60.8*	89.7	3.1*
Deutsch (1097)	1958-61	44.9	100.0	49.6	10.2*	64.9*	90.8	3.6*
Gordon (309)	1966-67	78.3	86.1	49.8	10.1*	64.0*	--	2.6*
Gray-ETP (92)	1958	47.8	100.0	52.2	9.0	69.3	88.0	3.7
Gray-FOHV (52)	1971	67.3	59.6	44.2	10.2	66.1	--	2.3
Karnes (228)	1961-63	100.0	68.4	47.4	10.1	64.3	92.3	3.4
Levenstein (250)	1964-70	72.8	95.2	48.0	10.5	63.8	91.2*	2.5
Miller (271)	1964	79.0	90.0	53.1	10.8	63.4	92.4	3.1
Palmer (309)	1964	78.3	100.0	0.0	11.2	57.5	91.3*	2.2
Weikart-Perry (123)	1958-62	47.2	100.0	41.5	9.4	68.2	79.0	4.0
Weikart-CD (41)	1964-65	100.0	51.2	46.3	10.3	63.8	80.6	2.6
Weikart-Carnegie (56)	1968	39.3	41.1	53.6	10.7	--	--	1.7
Zigler (144)	1962-64	53.8	88.5	44.2	11.1	--	--	3.0
Median		67.3	90.0	47.4	10.3	64.0	90.8	3.0

Note. Calculations for mother's SES, pretest IQ score and number of siblings are based on somewhat less than the total sample size for most projects. SES measure is the Hollingshead Index of Social position, ranging from 11 (highest) to 77 (lowest social position). Mother's educational level is grades completed.

* Mean figure is based on less than 60% of the original sample. Beller collected these data retrospectively for the 1976 follow-up sample only.

Table 4

Background Characteristics of the 1976 Follow-Up Sample

Project (n)	Birth Year	% in Program	% Black	% Female	Mean Mother's Education	Mean SES	Mean Binet Pretest IQ	Mean No. Siblings
Beller (126)	1959	38.1	89.7	50.0	10.5*	60.8*	90.9	3.1*
Deutsch (97)	1958-61	64.9	100.0	50.5	10.3	63.2	92.0	3.9*
Gordon (107)	1966-67	77.6	94.4	53.3	9.9	65.1*	--	2.6
Gray-ETP (77)	1958	46.8	100.0	55.8	9.2	69.5	87.7	4.0
Karnes (168)	1961-63	100.0	64.9	51.2	10.1	64.0	92.3	3.4
Levenstein (186)	1964-70	71.5	95.2	46.2	10.5	64.0	93.6	2.6
Miller (141)	1964	77.3	92.2	53.9	10.7	64.0	93.2	3.2
Palmer (228)	1964	78.9	100.0	0.0	11.2	58.2	92.2	2.4
Weikart-Perry (123)	1958-62	47.2	100.0	41.5	9.4	68.2	79.0	4.0
Woolman (611)	1966-68	31.8	--	43.0	--	--	--	--
Zigler (144)	1962-64	54.2	89.1	44.2	11.1	--	--	3,0
Median		64.9	94.8	50.0	10.4	64.0	92.1	3.2

Note. Calculations for mother's SES, pretest IQ score and number of siblings are based on somewhat less than the total sample size for most projects. SES measure is the Hollingshead Index of Social position, ranging from 11 (highest) to 77 (lowest social position). Mother's educational level is grades completed.

* Mean figure is based on less than 60% of the original sample. Beller collected these data retrospectively for the 1976 follow-up sample only.

Table 5

Number of Children Receiving Stanford-Binet or Wechsler IQ by Age by Project

	Age in Years																
Project	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Beller			57	167	163	157	150	149									
Deutsch			403	635	740	232	27	85									(88)
Gordon		196	186	187	179				(90)								
Gray-ETP			86	90	87	82	80		80							(72)	
Gray-FOHV		43	51	48													
Karnes		15	224	208	118	119	112	69						112			
Levenstein	27	153	150	103	81	8	1		(76)								
Miller			247	244	254	244	225					(141)					
Palmer		291	280	255							(132)						
Weikart-Perry		95	123	121	120	119	117	117	114				(110)				
Weikart-CD		41	40	41	36	41	40										
Weikart-Carnegie		38			36	53											

Note. Numbers in parentheses are those who received WAIS, WISC-R or WISC IQ tests in 1976 Follow-up. All other numbers refer to those who received Stanford-Binet tests.

Table 6

Number of Cases with 1976 Follow-up Data by Instrument (Wave 3)

Project	Follow-up Age in 1977 (years)	Parent Interviews	Youth Interviews	School Record Form	Ach't. Tests	Long School Record Form	IQ Scores[a]	1976 Follow-up Sample[b]
Beller	18	112	116	119	115	115	--	126
Deutsch	16-19	73	88	54	44	---	88	97
Gordon	10-11	107	106	107	103	---	90	107
Gray-ETP	19	72	69	75	72	74	72	77
Karnes	14-16	165	156	153	143	105	112	168
Levenstein	7-13	98	75	154	115	---	75	186
Miller	13	141	141	139	134	---	141	141
Palmer	13	143	144	223	197	---	132	228
Weikart-Perry	15-19	106	104	123	96	---	110	123
Woolman	9-11	54	97	611	349	200	95	611
Zigler	13-15	---	141	144	142	---	143	144
Totals		1071	1237[c]	1902	1510	494	1058	2008

[a] Scores include WISC-R scores for Gordon, Gray, Karnes, Miller, Palmer, Woolman; WISC scores for Levenstein, Weikart; WAIS scores for Deutsch; and PPVT scores for Zigler.

[b] Follow-up sample n is the number of cases with data on at least one instrument.

[c] Total includes 141 Zigler cases who did not have Consortium Interview.

Table 7

Percent of Students who Failed to Meet School Requirements (Placed in Special Education Classes and/or Retained in Grade), Program vs. Control

Project (n)	% Program	% Control	Chi-square	*p* (2-tail)	Pooled *z*	Pooled *p* (2-tail)
Closely approximating experimental design						
Gordon (82)	39.1	61.5	2.25	.134		
Gray (55)	52.8	68.4	1.25	.263		
Palmer (221)	24.1	44.7	7.66	.006		
Weikart (123)	17.2	38.5	6.78	.009		
Median for Experimental Projects (481)	31.6	53.1			4.00	$<.001$
Quasi-experimental						
Beller (69)	45.9	50.0	.11	.737		
Levenstein (127)	22.1	43.5	4.47	.035		
Miller (125)	20.6	11.1	.89	.356		
Median All Projects (802)	24.1	44.7			3.59	$< .001$

Note. Data were collected when most of the children were in the following grades: Gordon - 5th grade; Gray - 12th grade; Palmer - 7th grade; Weikart - 4th grade; Beller - 12th grade; Levenstein - 3rd grade; and Miller - 7th grade. Certain control groups were excluded so that the program-control comparison would be as close to experimental as possible.

Table 8

Percent of Subjects Placed in Special Education Classes, Program vs. Control

Project (n)	% Program	% Control	Chi-square	p (2-tail)	Pooled z	Pooled p (2-tail)
Closely approximating experimental design						
Gordon (82)	23.2	53.8	5.10	.0244		
Gray (53)	2.8	29.4	8.16	.0044		
Weikart (123)	13.8	27.7	3.55	.0602		
Median	13.8	29.4			4.04	<.001
Quasi-experimental						
Beller (66)	5.7	3.2	.23	.6315		
Levenstein (125)	13.7	39.1	8.07	.0046		
Miller (109)	20.4	12.5	.55	.4656		
Median All Projects (558)	13.8	28.5			3.52	.001

Note: Data were collected when most of the children were in the following grades: Gordon - 5th grade; Gray - 12th grade; Weikart - 4th grade; Beller - 12th grade; Levenstein - 3rd grade; and Miller - 7th grade. Certain control groups were excluded so that the program-control comparison would be as close to experimental as possible.

Table 9

The Effect of Early Education on Binet IQ When Background Variables and Pretest IQ Are Controlled, by Number of Years After the Program

Project		Immediate Post-test	One Year After	Two Years After	Four Years After
Beller	b	7.42	4.32	5.33	6.68
	t	2.758	2.411	3.161	3.929
	n	109	105	101	96
	sig	.007	.018	.002	.0002
Gordon	b	3.47	5.59	5.27	5.34
	t	1.573	2.378	2.455	2.707
	n	196	186	187	179
	sig	.117	.018	.015	.007
Gray	b	12.52	4.15	.92	.741
	t	5.560	1.441	.255	.207
	n	54	52	49	49
	sig	<.001	.156	.800	.837
Levenstein	b	7.87	4.46	15.30	6.80[a]
	t	2.938	.931	3.717	2.185
	n	134	102	79	61
	sig	.004	.354	.0004	.033
Miller	b	7.42	0.31	1.15	-3.45
	t	3.233	.131	.417	1.298
	n	227	216	206	189
	sig	.001	.897	.677	(.196)[b]
Palmer	b	4.68	5.26		
	t	2.297	2.582		
	n	278	253		
	sig	.022	.010		
Weikart	b	10.27	4.22	3.96	.37
	t	5.422	2.358	2.126	.187
	n	121	120	119	117
	sig	<.001	.020	.036	.852
Pooled z^2		8.535	4.569	4.826	3.130
Pooled Significance		<.0001	<.0001	<.0001	.002

Legend. b = unstandardized regression coefficient for program-control status (effect of preschool on IQ after all other variables are controlled). t=t-value for b. n=sample size. sig=significance level for b (2-tailed). E.g., the first entry in the table shows an immediate post-test difference of 7.42 points between Beller's program and control groups.

[a] Based on WISC IQ.

[b] Figures in parentheses indicate controls did better than program children.

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METHODS OF EVALUATING PROGRAM OUTCOMES

Chapter prepared for Bernard Spodek (Ed.), Handbook of Research in Early Childhood Education.

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Evaluation has assumed increasing importance in early childhood education as program accountability has become a dominant theme in all areas. Funding agencies at local and federal levels require data for planning and demand effective, equitable programs at reasonable cost. Federally funded programs in health, human services, and education now mandate evaluation reports (see e.g. Head Start Program Performance Standards).

Inevitably, evaluation of early childhood programs occurs in a turbulent setting because it deals with value systems. All programs operate in social, political, and economic contexts, and the process of evaluating these programs shares the same milieu. The sometimes conflicting values and belief systems of evaluators, program planners, administrators, and the families of children in the programs make the process of program design and evaluation a political activity. As in any political arena, all interests must negotiate their own perceptions of program needs and compete for resources and credibility. For example, program effectiveness is a highly salient value in outcome evaluation. Program effectiveness must, however, compete for influence on program decision-making with other considerations, such as the political acceptability, feasibility, and ideology of the program. The political process involves accommodation and negotiation among these competing values in the research design and in utilization of the findings of evaluation studies.

Since the 1960's, evaluators of early education programs have been primarily concerned with children poorly served by the traditional schools -- low-income children, minority children, or children with special needs. As soon as children graduated from the first summer Head Start programs in 1965, national evaluations of Head Start were planned. The data now available from early childhood education programs such as Head Start, Follow Through, Home Start, and Title I programs are voluminous. Reviews and bibliographies of these data have also accumulated (cf. Brown, 1978; Mann, Harrell & Hurt, Jr., 1976; Bronfenbrenner, 1974; Gotts, 1973; Rivlin & Timpane, 1975). A new field of meta-analysis has evolved in an attempt to integrate and make sense of findings from diverse studies (Pillemer & Light, 1980; Light & Smith, 1971; Cook & Levitan, in press; Glass, 1978).

Beginning in the early 1960's, a number of early childhood investigators independently conducted early childhood programs for research and demonstration purposes. Their infant and preschool programs, designed for low-income children, had diverse curricular approaches and were located in urban and rural sites (for details see Parker & Day, 1977 and Selected Bibliography in Consortium, 1978). Some programs were home-based, others were center-based, and some combined a center program with home visits. In 1975, partly in response to the negative findings of the Westinghouse Report, the investigators formed a collaborative research group -- The Consortium for Longitudinal Studies -- in order to study the long-term effects of their early childhood programs. Members of the Consortium are: E. Kuno Beller, Richard Darlington, Cynthia and Martin Deutsch, Robert E. Jester, Irving Lazar (Chair), Susan Gray, Merle Karnes, Phyllis Levenstein, Louise B. Miller, Francis Palmer, David Weikart, Myron Woolman, and Edward Zigler. Ira J. Gordon was an active member until his death in 1978. The members pooled the data they had collected independently prior to 1975 and then conducted two common follow-up studies: the first in 1976-77 when their subjects were 9 to 19 years old, and the second in 1979-80. The group members delegated the coordination of the collaborative follow-up studies of 1976 and 1979 and the analysis of pooled data to investigators at Cornell who had no vested interest in a particular early childhood education approach. The authors of this chapter, along with other colleagues, form the Cornell group.

The pooled analyses of the Consortium for Longitudinal Studies have demonstrated long-term positive effects in cognitive and non-cognitive areas (see Consortium, 1977; 1978; in press; Darlington et al., 1980). Individual Consortium investigators have published reports as well (see selected bibliography for each Consortium investigator included in the reference list).

[illegible] work with the Consortium for Longitudinal Studies utilized traditional methods of outcome evaluation. In this chapter we will discuss the outcome evaluation approach in the broad context of evaluation research, describe the basic tasks in conducting traditional program outcome evaluations, and then focus on certain methodological issues. These issues include (a) selection and scheduling of outcome measures; (b) establishing comparisons (selection, recruitment, and assignment of subjects); (c) hypothesis testing and estimation; (d) validity, power, and generalizability; (e) the nature of conclusions; (f) randomization and statistical control of group differences; and (g) attrition problems.

Overview of Evaluation Research

The process of evaluating social programs is variously termed evaluation research, policy research, action research, field research, applied research, program evaluation, or simply evaluation. Numerous textbooks and anthologies are available (cf. Caro, 1977; Cook et al., 1978; Coursey et al., 1977; Glass, 1976; Guttentag & Saar, 1977; Sechrest et al., 1979; Schulberg & Baker, 1979; Suchman, 1967; Weiss, 1972; Wholey et al., 1970) and journals with titles such as Evaluation and Evaluation Quarterly have proliferated. A comprehensive handbook for the application of measurement and evaluation to early education has just been published by Goodwin and Driscoll (1980). Campbell (1979) sees this as the emergence of a new discipline involving participants from many backgrounds, including economics, sociology, political science, education, and psychology, in addition to specialists from human service professions. Currently, evaluators are deeply involved in defining their unique area of expertise.

Evaluation can be broadly defined as the process of making judgments about social programs based on systematic data collection and analysis. The overall purpose of program evaluation is to inform policy at various levels of program management, planning, and development. It is intended to be of practical use in decision-making and policy formulation, but may extend beyond the immediate programmatic level to basic theoretical concerns about the role of certain kinds of experiences in child development. Evaluation data may thus affect conceptualization about the developmental process (Evans, 1974).

Evaluation research differs from other research in its emphasis on making judgments and its relationship to decision-making. Research questions for evaluation are derived from the program itself rather than a theoretical concern. In a sense, then, evaluation research begins with the independent variable (the program), whereas other research typically begins with the dependent variable (an area of interest). Another distinguishing aspect of evaluation research is that it attempts to apply research methods in an "inhospitable action context" (Weiss, 1972). It involves mostly open systems with multiple causation and a search for unbiased estimators of program effects in a setting that is not "noise-free" (Cook & Campbell, 1979). Thus the evaluator can expect logistical problems, interpersonal conflicts, and methodological compromises to an extent not seen in other research.

Purposes of Program Evaluation

Organizations require information to maintain effective internal operations and an effective relationship with the external environment. Depending on the level of organizational development and the resources (time and money) available, an evaluation serves several overlapping functions. First of all, evaluators can focus on collecting information on the internal operation of a program for system resource management. A second function might be to collect data on service delivery in order to improve client utilization of the program. A third focus might be on treatment effects on clients or community impact effects. In addition, evaluation studies can have a dissemination function to permit replication of programs, and a theory-building function to evaluate the assumptions underlying programs (cf. Attkisson & Hargreaves, 1979; Coursey et al., 1977).

The two major types of evaluation research, outcome and process evaluations, primarily serve two distinct purposes. Outcome evaluations are designed for "summative" purposes; process evaluations are designed for "formative" purposes (Scriven, 1967). Formative (process) evaluations focus on programs in operation and feed back into the cycle of program development; summative (outcome) evaluations judge a program at the end or at a critical decision point.

Evaluation data are expected to influence policy decisions. The most commonly held expectation is that evaluation will be instrumental in drastic go/no-go decisions such as whether a program should continue or be terminated. It is ironic, as Weiss (1972) and others have pointed out, that although evaluation methods may be appropriate for this type of decision-making, they are rarely used to terminate a program unless the decision serves political ends (Datta, 1976; 1980; Zigler & Valentine, 1979). In the case of the outcome evaluation completed by the Consortium for Longitudinal Studies, positive findings played a role in the Congressional decision to increase the appropriations for Head Start (Comptroller General, 1979). In general, evaluations are more often used to aid in decision-making about either improving program practices or achieving better allocation of resources among program strategies.

The purpose of the evaluation study determines the design and methodological approach that will be used in an evaluation. In the next section we present a brief overview of evaluation models, so that the outcome model can be placed in the context of alternative perspectives.

Evaluation Models

The evaluation literature offers many typologies of research approaches. Indeed, there are probably as many listings of evaluation models and research strategies as there are authors on the topic. A typical listing might include seven evaluation models: the outcome model, the system analysis model (Wildavsky, 1979), cost analysis model, descriptive and quality assurance model (outside review), program planning/management model, the Goal Attainment Scaling model (Kiresuk & Lund, 1979), and legal adversary model (Levine & Rosenberg, 1979). Other authors would provide different typologies (see e.g., Goodwin & Driscoll, 1980; Guba, 1977). The variety of evaluation models reflects an ongoing methodological debate between the mainstream "outcome" or "goal" model view of program evaluation and alternative perspectives.

The goal model to be discussed in this chapter involves the following activities: program goals are determined, the primary goals are translated into measures to indicate goal achievement, and outcome data are collected which compare a treatment to a control group or compare the treatment group to a certain standard. This approach attempts objectivity; it assumes that programs are designed to achieve specific ends, that success can be measured by the extent to which ends are achieved, and that policy decisions will (or should) be made according to objective data provided by evaluation (Wergin, 1976).

Critics argue that the goal model ignores organizational issues. In addition to their official goals for children or parents, programs are organizations that must adapt to their social, political, economic environment and acquire the resources necessary to continue the program. To tap these other functions, "goal-free" evaluation models have been proposed. These alternatives to the goal-model, termed process or system models, use other criteria to judge the effectiveness of an early childhood program, such as success of a program in acquiring resources. The naturalistic approaches of Stake (1967, 1975), Guba (1978), Stufflebeam (1968), Guttentag (1979), and Scriven (1967, 1974) have received wide attention as alternatives to the outcome model. Goodwin and Driscoll (1980) provide a good summary of various evaluation frameworks and note their application to early childhood education. Although we feel process models and naturalistic methods have an important place in evaluation research, in this chapter we will focus on the use of quantitative methods in outcome studies such as the Consortium Study.

There are few rules to guide an investigator in the selection of appropriate research strategies. There is obviously no one best way, and as Weiss (1972) points out, a compromise must be found between the ideal and the feasible, given the program and situational constraints of time, place, people, and money. A basic research primer like Selltiz et al. (1976) is a good source for general research design rules. Maynard-Moody and McClintock (in press) provide an exploratory attempt to study the appropriateness of evaluation designs and methods. They suggest that awareness of the organizational context, particularly the degree of uncertainty about program goals and the level of causal knowledge in a given program area, can provide guidance in choosing an evaluation strategy.

The particular model and methods utilized in program evaluation depend on the purpose and scope of the study, and also on the orientation of the evaluators. At every stage of the study, the underlying research paradigms play a strong role. Two major paradigms, the qualitative and quantitative, have divided the evaluation field into two camps. The labels "quantitative" and "qualitative" imply both specific data collection techniques and a particular conceptual frameowrk or set of assumptions (Reichart & Cook, 1979). The quantitative paradigm comes from the experimental tradition in physical sciences and psychology. Quantitative research is outcome-oriented; assumes a stable reality; uses an objective "outsider" perspective; collects "hard" replicable data; and is concerned primarily with reliability and generalizability. The alternative approach, qualitative evaluation research, derived primarily from social anthropology in response to dissatisfaction with quantitative approaches. Qualitative research is process-oriented, dynamic, and humanistic; uses a subjective "insider" perspective; collects "real and rich" naturalistic observations; and is concerned mostly with validity. Qualitative research is characterized by the single case study which is not generalizable.

Some evaluators have chosen to view the two approaches as incompatible. Others, however, advocate the use of a wide array of conceptual and methodological tools from both qualitative and quantitative approaches, depending on research needs. Recently, bridges instead of walls have been under construction between the two approaches (cf. Campbell, 1979; Filstead, 1979). For example, the naturalistic and holistic attributes of the qualitative paradigm can be combined with characteristics of the outcome model from the quantitative paradigm. Only tradition may prevent a researcher from mixing and matching qualitative and quantitative attributes to find the most appropriate approach for a research problem (Reichart & Cook, 1979).

Combining qualitative and quantitative approaches serves several purposes. First of all, evaluation research usually has multiple purposes and thus a variety of methods is appropriate. Secondly, the two approaches can build on each other to offer more complete insights. In addition, because all measures have biases, multiple techniques enable a researcher to "triangulate"; that is, to converge on understanding the same event with different methods (Denzin, 1970).

We turn now to some general methodological issues, beginning with the basic tasks that are required for outcome evaluation.

Basic Tasks in Outcome Evaluation

The traditional goal model approach to outcome evaluation involves a basic set of tasks. The first task is to determine the general program goals and the specific program objectives. From these, the primary goals for study are selected and then operationalized into outcome measures. Another major task is to identify an appropriate population to test the hypothesis of program effectiveness. From this population, subjects are selected, recruited, assigned and maintained in the program and, usually, in control groups. A data collection system must be designed to estimate both immediate and later program effects. Data collection instruments and procedures are selected to obtain valid and reliable indicators of baseline, intervening, and outcome measures. Procedures to ensure protection of subjects' rights must be instituted. Actual data collection must be monitored for consistency and accuracy. The characteristics of early childhood programs in operation must be specified and then monitored to determine if the program is implemented as planned. Analyses of the outcome data compare measurements influenced by the program with measurements not influenced by the program in order to ascertain the program's effect. Results are disseminated to sponsors of the study and utilization of the findings is expected.

In practice, the order in which these tasks are performed varies from study to study, as does the amount of control the evaluator actually has over the tasks.

In this section we will address some of the issues involved in conducting such an outcome evaluation. The topics we have selected are, for the most part, ones which have concerned us in the course of our Consortium study. For instance, we will not discuss documenting program implementation despite its obvious importance for outcome evaluation. Each of the projects was carefully implemented and documented prior to our participation; our role in the Consortium did not begin until long after the actual programs had ended. Thus, our Consortium experience gave us little new insight into this particular issue. The issues we will cover include: selection of measures; creation of comparisons; hypothesis testing and estimation; validity, power, and generalizability; the nature of conclusions; randomization; and attrition.

Measures

Perhaps the most politically sensitive and controversial aspect of evaluating program outcomes is what to measure and how to measure it. Regardless of the evaluation model or strategy chosen, appropriate indicators must be agreed upon to describe and understand the realtionship between programs and outcomes. When a goal model approach is selected, the theoretical constructs underlying program goals need to be operationalized in order to create outcome measures.

Specifying Program Goals and Outcome Criteria

Specification of goals and of the criteria used to judge success creates problems because both goals and outcome criteria are value statements. Consensus can be difficult because of the diversity of vested interests with conflicting values and priorities. There is some evidence that the very act of specifying outcome criteria can have an influence on program implementation such as when the goal of teaching becomes improving the test scores themselves (see e.g., Campbell, 1979 for examples of corrupting effect of quantitative indicators).

The goals of early childhood programs are value statements reflecting differing educational philosophies and theories of learning. Thus, each philosophical viewpoint or paradigm has characteristic goals for their children. For example, early childhood programs with a development-interaction paradigm might specify their outcome goals in terms of fostering psychological development, personal autonomy and self reliance, and integration of thought, feeling, and action (Evans, 1974). The short-term objectives might focus on attentional responses, task orientation, and motivation to learn. The long-term global aim might be to encourage a positive attitude toward learning. This viewpoint would caution against concentration on academic goals as ends in themselves at the expense of genuine intellectual growth.

Follow-Through evaluation provides an example of an outcome evaluation study which focused on specified program goals (Stebbins et al., 1978). The Follow-Through evaluation studied 22 program models which probably reflected 22 different viewpoints of developmental psychology. The stated goals and objectives of the program models were used to classify the diverse programs along two dimensions: first, the models were differentiated in terms of amount of emphasis placed on learning basic skills, on cognitive conceptual skills, or on self-esteem and sense of control; and second, the models were classified according to the degree of structure (low to high) in the curriculum.

Head Start provides an example of the evaluation problems created by multiple goals. The original program goals of Head Start included efforts to meet the health and nutritional needs of children from low-income families, to involve their parents and the community in fostering children's development, and to provide a happy transition from the home to a group setting. Emphasis was placed on the children's personal and social development, curiosity, motivation, trust in others, and independence (Datta, 1976). However, in order to sustain widespread support of varied constituencies, Head Start had a multiplicity of objectives ("glorious goalfulness"): in addition to its goals in child development, Head Start was also designed to be an employment program and a community action agency. The problem for Head Start evaluators became which goals had highest priority and which outcome criteria would be the "best" measure of Head Start's effectiveness.

Types of Outcome Measures

The selection of measures to assess outcomes of early childhood programs is both critical and difficult. Costs, benefits, and negative effects are important outcomes to assess. The task of the investigator is to link the implementation of the early childhood program (inputs) to some later observation (outcomes). In evaluation research both the treatment (cause) and observation (measure of effect) are complex products of many determinants. Stated more formally, measures are imperfect indicators with random error and systematic bias. As in other research, evaluation research involves the philosophical issue of causality and methodological issues of validity (Cook & Campbell, 1979). Evaluation research, however, is particularly constrained by what is feasible to measure and what is meaningfully related to goals of early childhood programs.

Early education programs for low-income children typically have as one of their goals the stimulation of cognitive development and the subsequent improvement of children's later school performances. Standardized intellectual tests are usually selected to measure these outcomes (Horowitz & Paden, 1973). Despite advantages of reliability, validity, and ease of administration, the use of standardized measures has been heavily criticized. Intelligence tests in particular have been attacked for cultural bias and as an inadequate sampling of the skills and abilities of low-income children (Kamin, 1974). Zigler and Trickett (1978) criticize the use of intelligence test scores alone as relatively uninformative and, at worst, misleading. They propose instead using a social competence index that includes both individual intelligence tests and molar social expectancy variables, such as school attendance and incidence of juvenile delinquency. Bronfenbrenner (1979) notes that outcome measures such as standardized intelligence and achievement tests are ecologically constricted, measuring a narrow range of behavior. He has described developmental psychology as the "science of strange behavior of children in strange situations with strange adults for briefest possible periods of time" (1979, p. 19). Moreoever, the validity of standardized measures depends on the extent to which the children share the experimenters' definition of the specific testing situation. Measurements are "ecologically valid," according to Bronfenbrenner, only insofar as the environment experienced by the subject has the same meaning as that assumed by an investigator (1979, p. 29).

Interviews and tests have also been criticized because they intrude as a foreign element into the natural setting, elicit atypical roles and responses, and are limited only to subjects who cooperate. Some researchers have advocated the use of unobtrusive measures, such as archival or written records, as an alternative strategy for gathering information (Webb et al., 1966). Such measures are also termed "nonreactive" because they do not contaminate the response.

Whether a measure is reactive or nonreactive depends on the specific actors in the setting: a particular measure is not inherently reactive or nonreactive. For example, data on assignment to special education classes collected from school records constitute a nonreactive measure vis-a-vis the children who participated in an early education program. Similarly, these data are nonreactive vis-a-vis the school if they are used to evaluate an early education program which was distinct from the school. However, if this measure is used to evaluate a program within the school making the assignments, then the data could be reactive vis-a-vis the school; if school officials know they are being evaluated on this measure, this knowledge may alter either the pattern of assignment to special education or the manner in which they record assignment.

Tests and interviews are always reactive relative to the subject since they depend on the subject's reaction to the testing interview situations. These measures may also be reactive relative to the program being evaluated. If, for example, program personnel know their program will be evaluated by the Stanford-Binet IQ test, they may include the Stanford-Binet items as part of the curriculum. Thus, for each proposed measure, the evaluator must judge the extent of reactivity for the subjects, for the program being evaluated, and for the person or agency which actually records the measure.

In addition to cognitive goals, many programs have goals for children's personal and social development, such as enhancing self-esteem, increasing achievement orientation, and facilitating the transition into the peer group and the classroom setting. In this area, there continues to be a lack of well-constructed and well-validated instruments that measure non-cognitive outcomes (for suggested sources, cf. Goodwin & Driscoll, 1980).

Another criticism of early education program evaluations, implicit in Bronfenbrenner's (1979) model for experimental ecological research, is the restriction of outcome measurements to attributes of the child. In order to be truly indicative of development, measures should assess the program's impact on the multiple contexts in which the subject lives, such as his/her family, school, and community.

Multiple Measures

Most authors recommend the collection of multiple measures at several time points for a number of reasons. First of all, we know that our measurements are only indicators of an unmeasured underlying construct. Multiple measures, perhaps both quantitative and qualitative, provide additional confirmation of observations and raise our level of confidence in our findings. The various measures can supplement and cross-validate each other. This process is termed "triangulation" (Denzin, 1970). Collecting multiple measures of the same construct also provides an opportunity for creating indices. An additional reason for assessing several outcomes is that programs can have multiple objectives and effects.

We used four types of measures in the Consortium data. First, we utilized IQ tests individually administered by the project. These tests were reactive for both the subject and the program, but had the advantage of being well-known standards for judging success. Second, we collected data on assignment to special education and grade retention from school records. These data were nonreactive for the children, because collecting this information required no reaction by subject to researcher; for the early education program, since this criterion was not chosen until after program termination; and for the schools, since the schools were not being evaluated. School record data were more ecologically valid than test scores since they indicated the ability of the child to meet the minimal demands of his/her school. School success was thus measured in a visible and meaningful way in the child's own ecological context. In addition, because of the crucial impact school actions may have on a child, these records were also important as a reflection of the interaction between the child and the school. Comparisons across school systems should not be used, therefore, for detecting individual differences because they may primarily reflect school policy differences instead.

For our third measure we used scores of school-administered achievement tests collected from school records. These proved extremely difficult to analyze for several reasons: different schools and projects used different tests; identification of the test (e.g., battery, level) was incomplete; and different types of scores were recorded in different schools (e.g., grade equivalent, percentile, standard score). In general, our experience suggests that for evaluation purposes, standardized test data should come from tests specifically administered for the evaluation rather than drawn from school records. Garfinkel's (1967) observation that good administrative record-keeping is very different from good research-oriented record-keeping seems to apply here. The final measure we used was data from interviews with both the mother and the youth. Interestingly, we found that some subjects gave responses that differed from school record information (e.g., on high school graduation). Multiple data sources improved our confidence in the accuracy of our measures. The multiple measures we collected enabled us to assess various domains of cognitive and non-cognitive functioning.

Scheduling of Measurements

Another important consideration in the selection of outcome measures is the timing of the assessments. The effects of early education programs can be investigated at multiple time points. Ideally, evaluators should use multiple reporting schedules: early measurement, immediately following program participation; at a subsequent intermediate time to test the reliability of early estimates; and a later assessment to test the long-range effects (Bernstein, 1976).

As Cook and Campbell (1979) suggest, the scheduling of outcome measures can be one of the most important tools for detecting effects and for attributing effects to the treatment. The formulation of the research question and the theoretical link between the program and its effects helps to determine the appropriate postprogram time(s) to measure outcomes.

Planning for short-term before/after evaluations as well as long-term assessments can satisfy the needs of both policy makers who require quick reporting of results and evaluators who prefer more certainty. Long-term evaluations are rarely possible, but can prove surprising (see Nagel & Neef, 1979, for example of positive effects of the New Deal on farmers who were observed 30 years later). In our case, the Consortium was fortunate to be able to follow subjects into high school and beyond and found long-term positive effects in contrast with disappointing earlier assessments.

Establishing Comparisons

Evaluation is essentially a matter of comparison. A program can be evaluated only in relation to something else. This "something else" is usually either another program or "what would have happened without the program." Successful program evaluation depends first upon the conceptualization of a comparison which is theoretically relevant, and second, upon the creation of an empirical comparison which accurately reflects the conceptual comparison.

In order to make inferences about whether a program caused certain outcomes, an evaluator must compare a set of outcome measurements to at least one other set of measurements. The purpose of evaluation design is to create an empirical comparison for which the observed differences can be said to have been caused by the program under study. In other words, the evaluation design should attempt to create sets of measurements which are comparable in all respects except that one set was influenced by the program and the others were not. This is done by systematically eliminating rival explanations for observed differences between the comparison sets of measurements; and alternatively, by eliminating rival explanations other than program failure for a lack of differences. This elimination of alternative explanations allows the evaluator to say with reasonable certainty that the program caused the differences. Alternative explanations are often referred to as "threats to validity" (Campbell & Stanley, 1966; Cook & Campbell, 1979).

The two major methods of eliminating or minimizing alternative explanations are (a) the selection-assignment procedure and (b) the statistical procedures for controlling measured nonprogram-related differences. The former operates to ensure the comparability of the sources of the sets of measurements; the latter functions to adjust the measurements themselves for other measured differences in the sources. Often both methods are utilized in conjunction. This section focuses on selection-assignment procedures; other sections will address related methodological issues.

Selection-Assignment Procedure

In the Consortium study, the present authors were not involved in designing the procedures for selection and assignment. At the time the Consortium was formed, the constituent projects had already completed posttest evaluations of their participants and had published reports. Our exposure to the selection-assignment process was from an entirely different perspective: our task was to analyze the procedures used in each of the 14 original projects in order to determine to what extent the results could be pooled across the projects. In effect, we performed a post-hoc comparison of 14 selection-assignment procedures. Such comparisons are relatively rare (one was performed for randomized field experiments by

Conner, 1978), but can be a very useful way to discover the problems of evaluation design. Our analysis led us to conclude that published evaluation reports rarely describe the selection-assignment procedure in sufficient detail to enable a reader to understand the procedure and make a fair judgment about its validity. This may be due to two factors: a general reluctance on the part of both journal editors and readers to devote much space to the mundane topic of sample creation; and a belief that a few sentences can adequately summarize the entire selection-assignment procedure. Here we will attempt to treat the procedure as a whole, utilizing some of the insights we derived from our analyses and the reflections of the Consortium project staff members about their own procedures.

The goal of the selection-assignment procedure is to create two or more sources for sets of measurements in such a way that these sources are equivalent in all respects except exposure to the program. Usually these sources are two groups of people, although in some designs the sources can be different points in time from the same group of people (e.g., the single group pretest-posttest design or, more generally, the single group repeated-measures design). The first topic to consider, then, is the relative advantage and disadvantages of using only one group of subjects as the source of measurements.

A one-group design involves only the children who participate in the program. You may assess the children both before and after the program (a one-group pretest-posttest design), or you may assess them only at the end of the program (a one-group after-only design). The after-only design is the simplest of all, but it has the obvious disadvantage that there is simply nothing with which to compare outcomes. If a group of third-grade children are reading at fourth-grade level at the end of the program, how do you know they weren't simply an exceptional group of children, and that their outstanding performance has nothing to do with their participation in a preschool program?

One-group pretest-posttest designs control for this obvious source of error by providing a pretest comparison to the posttest. Unfortunately, they also have a number of weaknesses. A difference between pretest and posttest scores might be caused by any number of factors, including:

1. External events unrelated to the program. A new and highly effective educational television program might begin about the same time as the experimental program. Or if the experimental program is only one of several programs in a public school, some other school program might improve dramatically at the same time. These are just two examples; the variety of external events, and their possible effects on measured program outcomes, is virtually infinite.

2. Consequences of time. For programs lasting weeks or months, the major factor in this category is maturation of the children involved. For programs of a few hours, time might have a negative effect: increased hunger or fatigue between pretest and posttest might result in impaired performance. But for programs of a few days, adaptation to the program environment might substantially increase scores from pretest to posttest.

3. Effects of pretest. The experience of taking the pretest can affect scores on the posttest. Positive effects may include practice and elation; negative effects may include fatigue, boredom, or embarrassment.

4. *Unintended effects of the experimental program*. It may become clear to the subjects during the program that they are part of an experiment; this can stimulate either improved or decreased performance on the posttest.

5. *Instrumentation changes*. Instruments used to assess subjects at posttest might differ from those used at pretest, even if they have the same name. Even if one uses the same standardized IQ or achievement test, if a program is of one or more years' duration, it is likely that this test will have different items and be standardized on different samples at the time of the posttest in comparison to the pretest. Thus, instruments may not measure the same traits at both time periods.

Numerous other alternative explanations of pretest-postest differences for a single group can be advanced; a thorough discussion of such factors is available in Cook and Campbell (1979). Many, although by no means all, of these alternative explanations can be eliminated by comparing separate groups of subjects: one that participated in the program and one that did not. For the remainder of this section, then, we will focus on establishing comparisons between at least two groups of subjects.

Discussions of the selection-assignment process usually focus only on the procedures used to assign subjects to groups. However, particularly in the case of program evaluation, the selection-assignment procedure as a whole must be evaluated in terms of whether it produces comparable groups. As used here, the selection-assignment process will refer to the entire process by which comparison groups are created: selection for the study, recruitment, refusal or acceptance, assignment to groups, and attrition. In most discussions, these procedures are categorized according to the type of assignment used (i.e., random vs. non-random). This emphasis is appropriate because the assignment procedure is the most important component of the entire process for ensuring comparability of groups. At the same time, many researchers mistakenly conclude that the selection-assignment procedure can be adequately conceptualized and reported by a description of the assignment; but type of selection, methods of recruitment, response of potential subjects to recruitment, units used in assignment, and attrition of subjects during the evaluation period must also be taken into account in ascertaining whether a given selection-assignment procedure has provided comparable groups of subjects.

This almost exclusive emphasis on assignment can be traced to the historical origins of evaluation research. Evaluation research designs were adopted from the research designs used in agricultural and psychological laboratory experiments where proper assignment procedures generally were sufficient to ensure comparability. With few exceptions, subjects entered and remained in the groups to which they were assigned. Corn did not refuse to be treated with certain chemicals; rats did not refuse to be subjected to certain types of stimuli. Even when human beings were the subjects of an experiment, they usually entered and remained in their assigned groups because (a) they had volunteered for the experiment and did not perceive any of the treatments as having great positive or negative value for them relative to any of the others, or (b) they were a captive pool of subjects whose position permitted them no choice (e.g., in a prison, mental hospital, or the armed services). Thus, the models of the selection-assignment process were developed under conditions in which assignment by the researcher basically assured the composition, and hence the comparability, of the groups.

The situation in most program evaluations is quite different. Programs are usually perceived by potential subjects as important; therefore, they will have an interest in whether they are placed in a group receiving the program or in a no-program control group. Furthermore, the ultimate decision to participate in a program lies with the subject. For most early education programs the parents of subjects can refuse to have their child enter the program. Such a decision is influenced by many factors, including the perceived desirability of the program and the manner of recruitment. Obviously, responses could differ, depending on whether the subject was asked to participate in a program or in a no-program control group. Thus, the actual selection-assignment experiences of the program evaluator do not always conform to a model in which subjects enter and remain in the groups to which they have been assigned.

Evaluators have responded to this challenge by identifying and categorizing such threats to validity. Cook and Campbell (1979) list a total of 13 possible threats to the internal validity of the design; seven of these threats result directly from the perceived importance of the program to the subjects and/or the participation choices of the subjects. These are: (a) different types of people can enter different groups if they are free to choose groups or if they are specifically recruited for different groups; (b) different types of people can drop out of different groups during the evaluation period; (c) if there are particular types of people in the different groups, external factors can affect each group in a different way for example, if most of the program group is on welfare and most of the control group is not, a change in welfare policy during the course of the evaluation could affect one group but not the other; (d) subjects in the control group might receive some of the benefits of the program by contact with subjects in the program group; (e) some or all of the control subjects may participate in a similar program offered by another source -- perhaps offered in response to the program under study; (f) control subjects may be motivated to perform better than usual because they know they are being compared to a group receiving a special program; (g) on the other hand, the performance of controls could be depressed by resentment that they were not assigned to the program group.

Obviously, specifying the assignment procedure does not rule out these threats. Such threats may also exist in laboratory experiments; however, they are less likely to occur and less likely to be important in laboratory experiments. The question is: For a given study, at what point do such threats become important enough that they must be dealt with specifically at every stage of the process?

The selection-assignment process can be divided into three types of action taken by the researchers: selection, recruitment, and assignment. Although some type of selection must precede the other actions, recruitment for the program can occur either before or after assignment. Any recruitment or assignment action calls for a response by the subject (i.e., to participate or not). We will describe each type of action in general and then illustrate how these actions can interrelate in a selection-assignment process.

Selection requires the definition of a target population or populations. The selection process is concerned more with generalizability of results than with comparability of groups. Evaluations rarely use a random sampling design from any population to select the subjects; hence, most generalizations extending evaluation results to wider populations are "common sense" rather than statistically based generalizations, built on perceived similarities between the population under study and a wider population. Nonetheless, these selection procedures are relevant to the comparability of groups if different selection procedures are used for the groups; for example, if a preschool group is compared to a group selected from the first grade classroom of the preschool graduates.

Recruitment is the actual contact with subjects for the purpose of convincing them to participate in some phase of the evaluation research. Recruitment may be the action which has been most neglected in the research design literature and in reports of actual evaluations. Recruitment actions can be classified in several ways. First, they can be classified in terms of technique: public advertisement, door-to-door canvassing, and letters sent to a preselected group (usually followed by personal contact). Second, they can be classified in terms of the reason given for recruitment: for a one-shot assessment, for a series of assessments, for participation in the program, for a chance to participate in the program, for delayed participation in the program, or for participation in another program not under study and not expected to affect the relevant outcomes. Third, they can be classified by timing: Are the program and control groups actually recruited at the same time or at different times?

Assignment is the process of placing specified subjects in specific groups. The major distinction in assignment is between random and nonrandom assignment (nonequivalent control groups). The advantages and disadvantages of random assignment have been described in detail elsewhere (Cook & Campbell, 1979; Boruch, 1974; Conner, 1978). Types of nonrandomized assignment include: assignment by need, by ability, by age, by first-come/first-served, by personal connection, or by geographic area. Types of assignment are also distinguished by unit. Often the unit is the individual subject, although it is sometimes an already constituted classroom (treatments assigned by classroom) or a geographic area, in which certain blocks, neighborhoods, or towns are assigned to program or control conditions. A final distinction sometimes made is whether or not the assignment procedure is completely specified or known to the evaluator (Cook & Campbell, 1979).

To show how these actions can be combined to form a selection-assignment process, we will give a few hypothetical examples for early education programs. All will involve random assignment; hence, the statement, "The program and control groups were formed by random assignment," would apply to all examples.

1. Public advertisements of a program are made in a given area. Applicants come to an office where they are told that the space is limited and that their names will be entered in a lottery to see if they will get into the program. They are asked to participate in a control group (i.e., to agree to periodic assessments) if they do not get into the program. The pool of applicants who agree to participate in either the program or control group are randomly assigned to either group and then notified of their

assignment. A few potential control subjects may drop out of the assessment process due to disappointment that they did not get into the program.

2. A list of preschool-age children is obtained from a local hospital or school district. Names on the list are randomly assigned to program or control groups. Program children are contacted and asked to participate in the program. Controls are asked to participate in a series of assessments, and not told about the program. Some parents of program children may refuse because they don't want outsiders to interfere with the raising of their children. Some parents of control children may refuse to participate because they feel that the assessments are just a nuisance that will not benefit them or their children.

3. Subjects are recruited door-to-door for a child health screening program which involves periodic assessments. The subjects who volunteer for this program are randomly assigned to program or control conditions. Those assigned to the program group are recruited for the early education program; those assigned to the control condition have already agreed to participate in the assessments for the health screening program and need not be recruited again.

All three of these examples involve random assignment at some stage. However, all three will not necessarily result in equally comparable groups. In the first example, those who agree to participate regardless of whether they get the program are likely to be rather unusual, and this may lower generalizability. However, if all subjects participate as agreed, regardless of assignment, the groups should be comparable. On the other hand, despite their previous agreement to participate in the study, some subjects may drop out in disappointment at being assigned to the control group. If refusals occur in the control group after assignment, a biased comparison could result; that is, the effect of the refusals could provide an alternative explanation for explaining results. In the second example, the groups are comparable at assignment (before recruitment). However, since the groups are recruited for different purposes (one for the program, the other for assessment only), people in one group may respond differently from people in the other group. Certain types of people may consent to participate in an early education program but not to participate in assessments, or vice-versa. Hence, an alternative explanation for results may be that the groups were not equivalent after recruitment because of a selection effect in which differential response to recruitment for different purposes destroyed the pre-recruitment equivalence of the groups. In the third example, the groups are comparable at assignment, after recruitment for the health screening program, but before recruitment for the early education program. If all those recruited for the early education program accept, then the groups are still comparable. One of the advantages of this design is that even if certain subjects refuse the early education program, they will still be given assessments as part of the health screeening program and their data will still be available to test for biases caused by their refusal.

It should be clear from the above examples that the assignment procedure alone does not ensure comparability. Although random assignment was used in all three examples, they may differ greatly in whether the groups which actually agreed to enter the evaluation design were comparable. Again, these examples emphasize that the assignment procedure should be regarded as one part of a total process leading toward group comparability rather than as the sole criterion.

Hypothesis Testing and Estimation

The ideal of traditional coutcome evaluation is to create a comparison between two or more sets of measurements in such a way that any differences observed between the groups are attributable to the program(s) being evaluated. The comparison is then used to determine: (a) whether the program affected selected outcomes, and (b) an estimate of the size (magnitude) of the effect of the program on the outcome measures. In order to obtain this information, the evaluation usually employs statistics. These two types of information correspond to the two basic types of statistical methods: hypothesis testing and estimation.

When an investigator attempts to determine whether a program has affected a given outcome, s/he is testing the hypothesis that the program has had no effect on the outcome. If this hypothesis is disproved, then s/he can say that the program had some effect on the program. By convention, the no-effect, or null, hypothesis is said to be disproved if the observed difference has less than a 5% probability of occurring by chance.

In attempting to determine the size of an effect, an evaluator can utilize various statistical estimation techniques. Evaluators generally compute single point estimates of effect-size in terms of a percent, a mean, a median, or a regression coefficient. For example, an estimate of the magnitude of the effect can be the mean gain in IQ points, or a difference in the percent of subjects assigned to special education classes. A simple technique is available, however, which would estimate a range of values within which it is likely that the true effect of the program will fall. This can be done by computing a confidence band. Although this technique is found in most elementary statistics texts, it has been underutilized in evaluation studies.

The hypothesis testing/estimation dichotomy concerned the Consortium study, as it does all evaluation studies, in terms of whether to emphasize the statistical significance or the size of effects. Statistically significant differences can result from very small effects -- with a large enough sample. A one-point IQ score difference can be statistically significant even if it is educationally meaningless. On the other hand, a large effect may not be statistically significant, due to small sample size or other considerations. Our solution to the dilemma was to interpret results in terms of both statistical significance and size of effect.

Validity, Power, and Generalizability

Program evaluators can make three types of errors: they can conclude that the program is effective when in fact it is not (a Type I error); they can conclude that the program is ineffective when it really is effective (a Type II error); or they can apply the results of the evaluation to other situations for which the results do not hold. Stated positively, the ability of the design to avoid these errors can be conceptualized respectively as validity, power, and generalizability. In thinking about some apparent problem with an evaluation design, it is always important to decide whether it affects validity, power, or generalizability.

Validity essentially means the likelihood that the design will not find a significant difference between groups when the program has no effect. Design characteristics which lower validity make it more likely that an evaluator will find significant differences when none exist. The convention in statistical decision-making is to reject the no-effect hypothesis when the probability of making this type of error is .05 or less. Validity, as used here, is a more restricted concept than as used by Campbell and Stanley (1973) or Cook and Campbell (1979). Here, it applies only to what the results of the evaluation will be when there is no real program effect.

Power refers to the likelihood that the design will find a significant difference when the program really has an effect. Design characteristics which lower power make it harder to find significant differences when real differences exist. Other considerations being equal, a reduction in sample size always lowers power. Cohen and Cohen (1975) demonstrate techniques for calculating power for a particular sample size.

Generalizability means that the findings of the study can be validly applied to other subjects, conditions, or programs which are similar but not identical. This is essentially what Campbell and Stanley (1966) refer to as external validity.

In our pooled Consortium analyses we chose to maximize validity at the expense of power. In order to establish credibility in the post-Westinghouse Report era, we felt that every analysis decision should be made according to the criterion of minimizing the probability of finding a significant result unless the program effects really existed. In many cases these "conservative" decisions increased the probability that we would not find a significant result even if a true effect existed. But this approach ensured that those results we found were genuine program effects.

The issue of generalizability has also arisen in the Consortium study, particularly with respect to whether the Consortium results can be generalized to Head Start. Since the Consortium programs were not a random sample of Head Start programs (in fact only one of the projects was a Head Start program), our results are not statistically generalizable to Head Start. Our findings are still relevant to Head Start, however, since many Head Start programs were modeled after the Consortium programs. One might say the Consortium work shows that Head Start can work, but not that Head Start does work.

Nonsignificant Results

The crucial dilemma of what to report when results are not significant can be approached by applying the concepts of power and validity within the framework of hypothesis testing and estimation of the magnitude of the effect.

A nonsignificant difference between experimental and control groups cannot be interpreted as "proving" or even "confirming" a null hypothesis of no difference between groups, because the data are also consistent with the hypothesis that there is a small but nonzero true difference between groups. Especially when a difference is nonsignificant, it is useful to calculate a confidence band on the true difference, in order to make some statement about how large the actual difference might be.

One dramatic example of this involved a treatment designed to prevent blindness in certain premature babies. It was established beyond reasonable doubt that the new treatment was at least partially successful in preventing blindness, but the question remained whether babies given the new treatment had a higher mortality rate than other babies. In a study cited by Day et al. (1979), 12 out of 40 experimental-group babies died, and 9 out of 45 control-group babies died. The difference was not significant ($p = .28$, two-tailed), so the treatment was approved for use. It was later found, however, that the new treatment did increase death rates, and it is no longer used. A confidence band calculated from the same data which yielded the nonsignificant difference suggests that the difference between mortality rates of the two treatments could be as large as 28%. If this figure had been calculated at the time, it might have suggested that more study was needed before adoption of the new treatment.

An alternative procedure, calculating the power of the analysis, is sometimes used when reporting nonsignificant results. The argument for this procedure is that the nonsignificant result should not be accepted unless the power analysis shows that the sample size was large enough to have detected a difference of important size. Power analysis may be appropriate before data collection; but after data collection, we prefer to use the confidence band rather than power analsis. The confidence band incorporates the data actually collected into the conclusions, while the power analysis is based entirely on an hypothetical analysis of what might have happened.

This difference between the two methods of analysis can perhaps be explained better if we introduce a word which is useful generally in discussing statistical results -- the word *consistency*. We can describe the ordinary p from a hypothesis test as a measure of the consistency between the data and the null hypothesis. The smaller p is, the less consistent the data are with the null hypothesis. Then a confidence band can be defined as the range of values consistent with the data, in the sense that the p measuring consistency would be over .05. A power analysis essentially shows what hypotheses are consistent with some nonsignificant result, while a confidence band can show what hypotheses are consistent with the particular nonsignificant result which was observed.

It is also important to remember that if a sample size is large, a difference between two programs may be statistically significant, but may still be too small to be of practical importance. This is an important reason for reporting the size of effects or differences as well as their statistical significance.

The Nature of Conclusions

A final set of concepts related to interpreting the results of hypothesis tests is important but rarely stressed. This set of concepts concerns making a distinction between "accepted conclusions" and "demonstrated conclusions." If you find a significant difference between a program group and a control group, you have demonstrated a difference between the two groups. This does not mean that you have proved the difference exists beyond all possibility of doubt; after all, statistical principles tell us that the probability was .05 that the difference may have occurred by chance. But you have still demonstrated the difference in an important sense. On the other hand, suppose you find no significant difference between the two groups. As most statistics books assert, this does not mean you have demonstrated that there is no difference. After all, the true difference might have been a small one, so that there was little chance of finding a statistically significant difference with your sample size. But we can say that the data are consistent with the hypothesis that there is no difference between the two programs. Furthermore, the hypothesis of no difference is simpler or more parsimonious than the hypothesis that there is some difference. Thus the hypothesis of no difference is the most parsimonious hypothesis consistent with the data. This is a very important notion in statistics and in science generally. Following Popper (1959), the eminent philosopher of science, we can never say we have proved a hypothesis. However, we can say that a certain hypothesis is the most parsimonious or simplest hypothesis consistent with the data we have observed so far. We thus tentatively "accept" that conclusion, knowing that we may well change our conclusion after more data are collected.

Thus we will say that a nonsignificant result leads to an "accepted conclusion" of no difference while a significant result leads to a "demonstrated conclusion" that a difference exists. In other words, a demonstrated conclusion is one arrived at by a formal test of hypothesis, while an accepted conclusion is one which has not been finally tested but which is the most parsimonious hypothesis consistent with the data. The distinction is particularly important in evaluation research since evaluators are often pressured to make statements about which they cannot test an hypothesis. Thus, most questions of generalizability of results can be answered only as "accepted conclusions" because evaluation designs rarely incorporate a sampling strategy that permits statistically valid generalization.

Another application of this distinction would be the use of parametric statistics with ordinal scales. In much evaluation research, the measure of program success is an ordinal scale (such as a 5-point Likert scale) rather than an interval variable (such as height or weight). Thus questions are frequently raised about the validity of comparing the mean score of children in an experimental group with the mean score of children in a control group. How can we usefully compare means if they are not valid measures for ordinal scales? One answer to this question makes use of the distinction introduced earlier betwen accepted conclusions and demonstrated conclusions. When a scale is genuinely interval in every sense, and the mean of a program group is significantly higher than a mean of a control group, then we can say we have demonstrated that the program is better than the control treatment. In other words, we have a demonstrated conclusion. On the other hand, if the scale is merely ordinal, this same conlusion is merely an accepted conclusion. But it is still a valid conclusion, and because there is a significant difference between the two means, it is the simplest or most parsimonious conclusion consistent with the data. We have not demonstrated that the program is better than the control treatment, but to conclude otherwise would require an assumption for which there is no evidence: that the ordinal scale is distorted in a particular way that produces a significant difference between means even though the program is no better than the control treatment. Thus, it is reasonable to draw conclusions from differences between two means, even if the scale used is ordinal.

Randomization and Statistical Control of Group Differences

Randomization has received a great deal of attention in the evaluation literature (Campbell & Stanley, 1966; Cook & Campbell, 1979; Boruch, 1978; Conner, 1978). There has been some confusion in the field, however, about what randomization is and how it fits into the evaluation design as a whole.

First, we must distinguish between random assignment and random selection. If you make a list of all the children in a school, use a random number table to choose 30 children for your study, describe two programs to the children, and let each child choose the program s/he wants, then you have used random selection. On the other hand, if you allow 30 children to volunteer for your study, and then use a random number table to place some of them in an experimental program and some of them in a control group, then you have used random assignment. In other cases you might have both, or neither.

Curiously, most statistics textbooks discuss random selection repeatedly but hardly even mention random assignment. However, for most evaluation studies, random assignment is far more useful than random selection. Furthermore, although both are difficult to achieve, random assignment is often easier to implement than random selection. Especially if the population from which you select is not even carefully counted -- for instance, the population of the United States -- random selection is virtually impossible.

Achieving random assignment in a field setting is a very difficult task. It is frequently difficult to obtain cooperation from subjects who know they are being randomly assigned to treatments, especially if one "treatment" is simply a control group which receives no special program. The problem is alleviated substantially if the only random assignment is between two or more attractive programs. Also, random assignment may be presented in a more acceptable manner to applicants if there is more demand for participation than can possibly be satisfied. Wortman and Rabinowitz (1979) found that a group of college students considered random assignment to be the fairest method of choosing subjects for programs in comparison with assignment on the basis of need, merit, or first-come/first-served. The investigator might also watch for situations in which random processes occur naturally, or at least without intervention by the investigator. For instance, Staw (1974) studied attitude changes among young ROTC men who had or had not been selected by the draft lottery in 1970 (Cook and Campbell, p. 372). Conner (1978) and Cook and Campbell (1979) list useful ideas about implementing random assignment designs.

Ordinary statistical methods are valid in the presence of random assignment, even without random selection. The nature of the conclusion, however, is changed somewhat. For instance, suppose you are given permission to conduct a research project in a nursery school with 10 children. These 10 children probably do not even approximate a random sample from any larger population; they might simply be the 10 children in that neighborhood whose parents both work. But suppose you randomly assign 5 of these children to an experimental group and 5 to a control group, and after several weeks you find that all the children in the experimental group score higher on a vocabulary test than any of the children in the control group. It can be shown that the probability this would happen by chance is about .004. That is, if the experimental program had no effect on vocabularies, then the probability is about .004 that the 5 children with the highest vocabularies would be assigned to the experimental group. This is because there are 252 ways that 10 children can be divided into two groups of 5 children each. Only one of these 252 possible ways involved putting the 5 children with the highest vocabularies in the experimental group. Thus, the probability this will happen by chance is 1/252, or about .004.

The population referred to by the significance test consists of the 252 possible ways of assigning ten children to two groups of five children each. In other words, the population is not a population of children at all, but rather a population of possible assignment patterns. Thus, the justification for the significance test is quite different from the justification given in most elementary statistics texts, but it is a real justification nonetheless, and it is far more relevant to most evaluation research.

If you have used random assignment without random selection and you find a significant difference between programs A and B, then you have proven quite rigorously that there is some difference between the effectiveness of the two programs for these children. It is then a matter of judgment to guess whether this difference applies to a broader population of children; but you have used statistical methods to show formally that there is some difference between the two programs. To put the matter in terms of validity and generalizability, random assignment is important for validity, and random selection is important for generalizability.

The distinction we established between demonstrated and accepted conclusions is useful in discussing the difference between studies using only random assignment and studies using both random selection and random assignment. In both cases a significant result means you have demonstrated that there is some non-chance difference between two programs. If you used random selection from some broader population as well as random assignment, you also have a demonstrated conclusion that there is a difference in average effects of the two programs for the population as a whole. But if you used random assignment without random selection, then you can still have an accepted conclusion that there is a difference in effectiveness of the two programs for the population as a whole. You have demonstrated that there is a non-chance difference between programs for the sample you studied. Thus, the most parsimonious hypothesis about the population as a whole, consistent with this piece of data, is that the same difference you observed in your sample also applies to the population as a whole.

We have discussed at some length how random assignment differs from random selection, and how this difference affects conclusions. Now let us focus on random assignment itself, and ask why it is important. To do this, we must first introduce the term "covariate." A covariate is a variable on which two groups may differ which is unrelated to the effectiveness of the programs being studied. For instance, if you let children choose which program they will enter, then it may be that the children who chose program A are more intelligent or more highly motivated than the children who chose program B. If this occurs, then intelligence and motivation must be regarded as covariates and their effects must be accounted for in evaluating the relative outcomes of the two programs.

There are several statistical methods for controlling covariates: partial correlation, analysis of covariance, and multiple regression. A much simpler method of control is matching of distributions. For instance, suppose you find that children who chose program A come from families with higher socioeconomic status (SES), on the average, than children who chose program B. Then SES is a covariate. Suppose you allowed all children to participate in the programs for which they volunteered. When you analyze the relative effectiveness of the two programs later, you may delete from the statistical analysis some of the higher SES children who chose program A, and some of the lower SES children who chose program B, to make the distributions of SES similar for the two programs. This is a fairly simple way to improve the validity of your analysis. Its obvious disadvantage is that the deletion of subjects makes it harder to find a significant difference between the two programs; that is, it lowers power. This is the major advantage of the statistical methods of control, such as multiple regression, over matching. Nevertheless, we wish to discuss some problems which arise with any method of control, and for simplicity we shall assume for the moment that you have used matching of distributions.

If there is no random assignment, then differences between groups A and B can arise in many ways. Regardless of whether the children themselves volunteered, their parents chose which group they would participate in, or some other process was used, there might be differences between groups A and B.

Suppose you match the distributions on IQ scores in order to eliminate that potential source of invalidity in your research design. A critic might still say that perhaps the two groups differ on SES, so you might match on that. Then another critic might say that perhaps the two groups differ on motivation, so you might match on that. In principle, critics could keep naming potential covariates indefinitely; it is impossible to match on all of the covariates, or even to measure them satisfactorily. Thus, no matter how complete and careful your matching, a critic can always point to other variables on which there was no matching or control.

How does the situation differ from this when you have used random assignment? After all, even with random assignment, differences between the two groups can still arise by chance. In fact, we know that the probability is .05 that, just by chance, there could be a significant difference between groups A and B on IQ. There is also a probability of .05 that there is a significant difference between the two groups on motivation, and another .05 for SES. A critic might argue, therefore, that it is virtually certain that the two groups will differ on some covariate. So what has been gained by random assignment? The answer to this question is found in the basic meaning of statistical significance. Even though it is almost certain that the two groups will differ on some trait just by chance, the probability is only .05 that they will differ on the dependent variable. This is true even when you have used no matching or control of covariates at all. In a study using random assignment in which no covariates were controlled by other means or even measured, there is better control of covariates than in the most carefully matched study without random assignment, all other things being equal.

This does not mean that with random assignment, there is no need for matching or other methods of controlling covariates. Suppose we have a design with random assignment, and we are wondering whether to match groups on covariates such as IQ and SES. Although we have argued that random assignment gives us good validity even without matching, it can be shown that matching can substantially increase power. That is, matching can substantially improve the chance of finding a significant difference between programs if a difference really exists. The same is true of the more complex methods of controlling covariates, such as multiple regression. Therefore, these methods are still useful with random assignment, but their major contribution is to raise power rather than validity.

Attrition

Some subjects drop out during the course of any longitudinal study. Some may move; some may become disatisfied with the program and drop out; some may refuse to be assessed a second time; some may even die. This process of attrition may be either random or systematic (e.g., caused by some aspect of the program or of the evaluation process).

For an evaluation design to be valid, groups must be comparable at the point in time when they are being analyzed. A design could have groups which were comparable at the beginning but due to systematic attrition are no longer comparable at post-test. Attrition always affects power, but may also affect validity or generalizability.

Attrition was a major concern in the Consortium study because the first Consortium follow-up in 1976 was generally conducted about a decade after subjects had left the early education programs. When the Consortium was formed, no one was certain whether we could find enough subjects to make a valid study. We addressed this problem on two levels: first each project staff attempted to find all possible subjects; and second, the Cornell staff (the authors) designed a system of analyses to detect attrition-related biases. Although the latter form the basis of our discussion, it was the former -- the determination of the project staff members -- that enabled the Consortium to overcome the problem of attrition.

In the Consortium study, this problem was in one respect greater but in another respect smaller than what one would anticipate for an evaluation involving only a posttest administered immediately after the program. Attrition was a greater concern for the Consortium in that finding the subjects after a lapse of several years was often extremely difficult. On the other hand, attrition was less of a concern because the factors that made a subject difficult to find several years later were more likely to be similar for program and control subjects than were the factors involved at immediate posttest. Attrition at immediate posttest is more likely to be caused by a reaction to the program (e.g., dropping out because of problems with the program); hence, the attrition rate is more likely to be different for the two groups. Such differential attrition is a greater threat to the validity of an evaluation than is sheer amount of attrition.

Although attrition has long been recognized as a major problem in longitudinal research, comparatively little thought has been directed to analyzing the attrition process, and even less to what to do if attrition is discovered. In this section we will discuss some definitional problems with attrition; delineate several types of attrition; discuss each type in terms of its effect on validity, power, and generalizability; and suggest some approaches to addressing the problem in the analyses of program outcomes.

Types of Attrition

Attrition is not a single phenomenon, the presence or absence of which can be determined by a single analysis. Rather, there are several types of attrition which can affect validity, power, or generalizability. Different rates of dropout between treatment and control groups constitute attrition; a difference between the dropouts and the final sample on some important characteristic also constitutes attrition. For the Consortium study, we developed five specific questions designed to detect different types of attrition:* (a) *Rate of attrition*. What percentage of the original sample was recovered in the follow-up? (b) *Differential rate of attrition*. Were there different rates of attrition for program and control groups? (c) *Main attrition effect*. Did the final samples differ from the dropouts on some important characteristic? (d) *Differential attrition effect*. Are different kinds of children selectively retrieved in program versus control groups? and (e) *Equivalence of final groups*. Did final program samples differ from final control samples on some important characteristic?

The first four questions ask to what extent the final sample accurately represents the original sample and reflect four distinct types of attrition. The usefulness of such information hinges primarily on the initial equivalence of the program and control groups. If the groups were equivalent initially, analyses of attrition can assess the extent to which they remained equivalent. The fifth question, while not an attrition question per se, directly addresses the question of the equivalence of the final preschool and control groups. Thus, differences found in answer to the fifth question are not always attributable to attrition, but may be caused by differences in the original groups.

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*A very similar scheme was developed by Jurs and Glass (1971).

Rate of attrition. The question of attrition rate asks what percentage of the original sample was recovered. Rate of attrition, by itself, affects only power. If a large percentage of subjects have dropped out, but no other attrition effects have been discovered, then it is reasonable to conclude that attrition was a random process that will not affect validity or generalizability. This question is not susceptible to tests of inferential statistics since it does not involve a specific null hypothesis.

Differential rate of attrition. Differential rate of attrition poses the question of whether different proportions of treatment versus control children dropped out of the sample. In an experimental design, a differential rate of attrition is a distinct possibility during the experiment: the treatment children would have more incentive than the controls to report for evaluation if they perceive the treatment as beneficial; therefore, they may have a lower dropout rate during the treatment than the control group. After termination of the treatment there is less reason to hypothesize a differential rate of attrition since the treatment group no longer has an added incentive to be available for evaluation.

A differential rate of attrition indicates that different attrition processes were occurring for program versus control subjects; hence, it poses a threat to validity as well as power. If the rates of attrition are quite different for program and control groups, it becomes doubtful that the groups are still comparable, even if no difference between the groups emerges on any measured variables.

The appropriate test for differential rates of attrition is a chi-square statistic from a 2 X 2 crosstabulation of program/control status by dropout status for each project. We used the uncorrected chi-square for all 2 X 2 tables (see Fienberg, 1978).

Main attrition effect. Main attrition effects pose the question of whether the dropouts differed from the final sample on some important characteristic. The existence of a main attrition effect does not affect the treatment-control comparison for the final sample, but it does mean that one cannot extend this inference to the original experimental sample. For instance, if the final sample was found to be of lower SES than the dropouts (e.g., the higher SES families had moved out of the area and couldn't be located), then any treatment/control differences found at follow-up would hold true only for the lower SES subjects and not necessarily for all subjects. Thus, a main attrition effect changes the referent population to which the findings can be generalized and lowers power as well.

To test for a main attrition effect, one must first identify preprogram variables on which it is important to know if the dropouts differ from the final sample. For the Consortium study, we focused on preprogram IQ score, mother's education, and socioeconomic status. Each of these variables becomes the dependent variable in a 2 X 2 analysis of variance in which program/control status and dropout status are the factors. The significance level of the dropout main effect in this ANOVA determines whether there is a main attrition effect. A simple analysis would be a t-test between dropouts and final sample on each preprogram variable.

However, we utilized the ANOVA approach because (a) it removes any variance attributable to a program-control difference and (b) the single ANOVA gives the information necessary for both this question and the following one.

Differential attrition effect. The question of differential effects is the most complex attrition question we have posed: it asks whether different kinds of children were selectively retrieved in treatment than in control groups. More specifically, the question is whether the difference between the preschool dropouts and final sample is significantly different from the difference between the control dropouts and the final sample. For example, this could occur if for some reason only the "best" experimentals and the "worst" controls were retrieved. Alternatively, one could find that the initial "best" treatment subjects had dropped out of the follow-up assessment program, while only the "best" control subjects were motivated to continue assessments. The differential attrition effect question investigates this type of effect by comparing four groups: treatment dropouts, treatment final sample, control dropouts, and control final sample, on selected characteristics. A differential attrition effect threatens both validity and power. It indicates that groups which were originally comparable on a specific variable are no longer comparable. The appropriate test for differential attrition effects is the significance levels of the program status (treatment/control)-dropout status (found/not found) interaction in the 2 X 2 ANOVAs described above.

Equivalence of final groups. As noted previously, the question of the equivalence of final groups is not an attrition question per se. It asks whether the final treatment group was equivalent to the final control group on important background measures rather than whether the final sample was equivalent to the original sample. Differences between final treatment and control groups cannot be ascribed automatically to attrition, because such differences may only be reflecting differences between the original treatment and control groups. Therefore, any differences must be explored further to ascertain whether they are caused by attrition or by differences in the original sample.

Nonequivalence of final groups, like nonequivalence of initial groups, poses threats to both validity and power. One can test for equivalence of final groups by using t-tests between the final sample program and control groups on the preprogram variables used in the main attrition effect and differential attrition effect analysis.

These five questions provide a framework for detecting the presence of attrition. As can be seen, not all types of attrition distort the program-control comparison. Loss of subjects alone only lowers power. Certain types of systematic attrition can operate equally in program and control groups, producing a main attrition effect which makes the final sample different from the dropouts but which does not alter the internal validity of the program-control comparison in the final sample. The other types of attrition -- differential rates of attrition and differential attrition effect -- indicate that the program-control comparison for the final sample may be biased. As a last step, the final sample group should be tested for equivalence on preprogram variables, since nonequivalence here, for whatever reason, will produce biased results.

This set of analyses will reveal a great number of the problems caused by attrition. However, the evaluator should be aware of the limitations of these attrition analyses. First, they are limited to the background variables which were collected. If there was differential attrition with respect to an unmeasured variable, it will remain undetected. Second, since the attrition analyses are an attempt to prove the null hypothesis, at most one can arrive only at an accepted, rather than demonstrated,conclusion that there was no attrition effect.

Stronger conclusions may be possible if the rate of attrition is small and the outcome variable is dichotomous. If a child drops out and has no posttest data, then it would seem that the investigator has no choice but to delete him or her from the analysis. Surprisingly, however, there is an alternative.

Suppose that the dependent variable, on which the child is to be evaluated after the program, is dichotomous -- for instance, "successful" or "unsuccessful" at a certain skill taught by the program. Assume that each child was placed in the category that would make the program look least effective; specifically, assume that children who dropped out of the experimental group were placed in the "unsuccessful" category, and children who dropped out of the control group were placed in the "successful" category. For instance, suppose 40 children complete an experimental program and another 40 complete a control program. Suppose 30 of the experimentals and 10 of the controls are measured as "successful" at the end of the program. Now suppose that three children had dropped out prior to the completion of each program. Assume that had they continued, the three from the experimental program would have been unsuccessful and the three from the control program would have been successful. Then the frequencies for the experimental program would have been 30 successful and 13 unsuccessful; and frequencies for the control program would have been 13 successful and 30 unsuccessful. The difference in success rates is still significant at the .00025 level (two-tailed) even after these subjects have been placed as they were. This is a very conservative procedure which of course is lacking in power if the number of missing subjects is at all large. Furthermore, its use with a continuous dependent variable might be unfeasible because in order to ensure complete validity, the missing subjects in the experimental group should be placed at the bottom of the range and those in the control group should be placed at the top. This would entail an extensive reduction in power. To use this procedure with a continuous dependent variable, it may help to dichotomize it in order to avoid too much loss of power.

In effect, this technique establishes a "worst case" program effect by showing what the effect would be if calculated on the assumption that all dropouts had outcome scores unfavorable to the program. Obviously, if there is a large percentage of dropouts, then the "worst case" would produce a negative program effect. If, however, the program effect is calculated in this way, one has set a lower limit on the program effect.

A final technique could be used if there is differential attrition or nonequivalence of final groups on a certain preprogram variable.

This technique would be to delete the dropouts from the analysis and to use the troublesom preprogram variable as a covariate in all outcome analyses. This would adjust for the measured bias, although it would systematically underadjust for the "true bias" due to measruement error in the covariate (Campbell & Erlebacher, 1975; Cook & Campbell, 1979). However, it would not adjust for the loss of power.

These techniques are only partial soluations to problems of attrition. The only effective way to minimize attrition effects is to make every effort to assess all possible subjects during posttest data collection.

Summary

We have presented an overview of issues in research methodology that can be applied to studying theoutcomes of early childhood education programs. These concepts are relevant to experimental and quasi-experimental studies such as our investigation of the long-term effects of Consortium early childhood programs. The outcome evaluation approach was placed in the context of evaluation research in general, and the basic tasks in conducting program outcome evaluations were described. We then discussed some general research design issues and applied them to outcome evaluation using examples from early childhood education evaluations, particularly the research of the Consortium for Longitudinal Studies.

We considered several methodological issues that we felt were most relevant to studying program outcomes. The first topic we considered was the selection of suitable measures, the specification of program goals and outcome criteria, and the scheduling of measures. We next addressed the issue of comparing outcome measures to other sets of measurements and offered suggestions for minimizing alternative explanations through appropriate selection, recruitment, and assignment procedures. The weaknesses of one-group pretest-posttest designs were considered in the section on establishing comparisons. In the next section we discussed the use of statistics in judging whether the program had a measurable effect and in estimating the magnitude of the effect (hypothesis testing and estimation).

We then discussed the ability of evaluation designs to maximize validity, power, and generalizability, and ex[illegible] the value of calculating confidence bands when results are insignificant. The nature of conclusions and the distinction between accepted and demonstrated conclusions were discussed and applied to ordinal scales. We also discussed randomization and the statistical control of group differences. Finally, we described types of attrition and offered approaches to addressing attrition problems.

We hope that our review of selected methodological issues provided both some general guidance and a useful reference list for a deeper investigation of issues raised.

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Differential Effectiveness of Early Education:
Testing for Long-term Interactive Effects

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The objective of this study was to explore the widely-held assumption that certain subgroups of children may benefit more from early education programs than do other subgroups of children. The question of differential effects of programs arises from the observation that a program does not exert a uniform effect on all participants. There is often considerable divergence in outcomes for individual program participants due to individual differences. In addition to investigating the average effects of a program,an investigator is then obligated to attempt to explain some of the variation in program outcomes by investigating how the program affect various subgroups of participants (categorized according to traits or attributes which may plausibly affect their response to the program). These considerations are questions of interaction effects. Stated formally, interaction effects occur when two (or more) variables have a joint effect on the dependent variables, e.g., when the relationship between educational treatment and educational outcome depends on individual characteristics of the learner (Cohen & Cohen, 1975).

In educational psychology, this area of research is variously termed Aptitude-Treatment Interaction or ATI (Cronbach & Snow, 1977; Snow, 1978), Trait-Treatment Interaction or TTI (Berliner & Cohen, 1973), or Attribute-Treatment Interaction (Tobias & Abramson, 1971). A growing literature on ATI has demonstrated that aptitude (trait or attribute) variables interact with treatment variables to predict subject outcomes. Attributes investigated have included personality, status, aptitude, and interest variables. For educators the eternal quest is the perfect match of educational treatment to learner characteristics. Bloom's Mastery Learning approach, for example, suggests a range of instructional alternatives to meet individual student needs (1976).

Evaluators and program planners in the early childhood education field share a concern that programs might be differentially effective. Bernstein (1976), for example, cautions that reports of overall results may misrepresent true effects for certain subgroups of children. Even though the number of individual level variables that might interact with treatment is very large, Bernstein recommends that the evaluator should at least consider some standard sociological variables such as age, sex, race, education, and income.

Interest in the interactive question goes back to the first national evaluations of Head Start of 1966 - 1969 (Datta, McHale & Mitchell, 1976). With the high optimism of the early days of Head Start, there was no doubt about the overall effectiveness of Head Start programs. Instead, the most fruitful questions were interactive questions: What kinds of experiences have what kinds of effect for which children? After the negative effectiveness findings of the Westinghouse report, the interaction question became critical: Was Head Start effective for certain subgroups of children (Smith & Bissell, 1970; Evans, 1973; Gotts, 1973)?

The literature on differential effectiveness of early education programs is sparse and contradictory. One explanation for inconsistent findings is that treatments obviously vary; the goals, curricular design, and implementation strategies of early childhood programs are extremely diverse. A particular program approach may be intrinsically more effective with a certain type of child. Some investigators have asserted that preschool programs are most effective for children most in need (Datta, 1979; Smith & Bissell, 1970) while others concluded that programs are least effective for children most in need (Bronfenbrenner, 1974; Herzog, Newcomb & Cisin, 1972). Smith and Bissell (1970) found that after correction for the higher SES in the Westinghouse non-Head Start sample black children, children from the South, and children from inner cities scored higher than non-Head Start children in measures of achievement at first grade. Bronfenbrenner (1974) concluded that:

> The available evidence doesn't justify the conclusion that disadvantaged children with the lowest IQ benefit most from intervention (p. 10). The children who profitted least from the program, and who showed the earliest and most rapid decline, were those who came from the most deprived social and economic backgrounds. Especially relevant in this regard were such variables as the number of children in the family, the employment status of head of household, level of parents' education, and the presence of only one parent in the family (p. 53).

In a reanalysis of data from the Westinghouse report, Barnow and Cain (1977) did not find SES effects, but the Stanford Research Institute, using a different Head Start sample, found slight SES effects in favor of relatively lower SES children (cited in Datta, 1979). Differential benefits have been reported for both boys (Miller, 1975; Herzog, et al., 1972) and girls (Beller, 1974; Seitz, 1977; Weikart, et al., 1977). No clear patterns have emerged thus far.

Methodological considerations may explain the lack of consistent findings. First of all, the particular subjects' attributes or aptitudes studied by various investigators may not be the most critical variables. Secondly, over the years, analysis methods have varied from using simple short-term pre-post gain scores to the use of complex interaction terms in regression equations. The analyses of differential effects presented in this paper represent an effort to explore these issues (Koretz et al,1978).

Data Source

The research reported here is part of a continuing data collection and analysis effort conducted by the Consortium for Longitudinal Studies (see Consortium, 1977, 1978, in press).* Present and past members of the Consortium include: E. Kuno Beller, Cynthia and Martin Deutsch, Richard Darlington, Susan Gray, Ira Gordon, Robert Jester, Merle Karnes, Irving Lazar, Phyllis Levenstein, Louise Miller, Francis Palmer, David Weikart, Myron Woolman, and Edward Zigler. In the 1960's, Consortium investigators designed and directed independent research and demonstration early childhood programs (see Table 1). The programs were home, center or combination home visit/center programs for preschool children or infants and their mothers. The investigators either randomly assigned subjects to treatment and control groups or recruited a comparison group with background characteristics to match the treatment group. Follow-up measures were collected for several years after program termination. The children came from low-income families in large cities and rural areas, most of the sample was black, and the average mother had not completed high school at the time of the preschool program.

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*See also publications of individual Consortium members listed in Selected Bibliography.

In 1976, the investigators collaborated in the design of a common follow-up study. The preschool graduates and their control or comparison groups (ages 10 to 19) were located, tested, and interviewed and their school records were examined. Characteristics of the follow-up sample are summarized in Table 2. Five types of follow-up data were collected in a common format: an individually administered intelligence test (Wechsler-Revised), interviews with participants and with parents (usually the mothers), scores on standardized achievement tests (generally administered by the school), and school record information.

Previous Main Effects Findings

Our main effects analyses to date are summarized here. More detailed presentations of main effects results can be found elsewhere (Consortium, 1977; 1978; in press; Darlington, Royce, Murray, Snipper & Lazar, 1980). We have examined four outcomes in our study of the main effects of Consortium early childhood programs: intelligence test scores measured at several time periods, school competence measured as special education assignment and retention in grade, standardized achievement test scores, and certain noncognitive outcomes particularly achievement orientation.

Our findings replicated results on IQ reported by numerous previous investigators -- large effects in tests given soon after the program, tapering down to smaller but statistically signifcant effects 3 or 4 years after preschool and vanishing thereafter. IQ scores, however, are merely indirect predictors of school success; our most important analyses concerned the effects of preschool on direct measures of school success.

Our dependent measures of school competence were whether a child had been retained in grade (held back) or assigned to special-education classes at any time during his or her school career.* Although local school districts have varying policies regarding special education classes and grade retention, the two indicators share the implication that the student has not met the school's requirements for normal progression. Therefore, for a third dependent variable, we combined the two indicators into a single composite measure distinguishing students who met from those who failed to meet the school's basic requirements; we counted as meeting those requirements children who were never placed in special education classes or retained in grade.

We found that children who attended preschool were more likely to meet the requirements of their schools than children with no preschool experience. The median rate of failure by that standard was 45% in the control group, 24% in the treatment group (pooled $p < .001$). For special education the median rate of assignment to special classes at any time in the school career was 29% in the control group, 14% in the treatment group (pooled $p < .001$). The median rate of grade retention was 25% in in the treatment group and 31% in the control group (pooled p =..018).

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*The category of special education included classrooms for children diagnosed as educable or trainable mentally retarded, learning-disabled, or emotionally disturbed. Classes for children with speech and hearing difficulties were not considered special education programs for these analyses.

Achievement test analyses have proved extremely problematic. We found only limited evidence that preschool graduates performed better than did controls. Analyses of the 1976 interview items provided evidence that preschool graduates were significantly more likely than controls to give achievement related reasons for being proud of themselves (see Koretz, 1978). Preschool program apparently had a significant effect on mothers' vocational aspirations for their children.

Attrition analyses indicated that attrition appeared to be essentially random introducing no known biases into our results. Less than 3% of the recovered subjects refused to participate in the follow-up.

The preceding brief summary of our main effects findings is intended to serve as an introduction to the interactive research questions addressed in this paper. The dependent variables used in both the main effects questions and interactive questions were IQ test scores and school competence measures.

Methodology

In asking whether an early education program benefits one type of low-income child more than another (e.g., boys more than girls), one could simply look at differences in outcomes for the different types of children who had received the program. This procedure, however, would not control for maturational differences which may have occured independently of the program (e.g., over a certain age span, low-income girls may progress more than low-income boys on IQ test-related abilities). Therefore, to more rigorously address the question of whether early education programs have differential effects, we included the "untreated" control groups into our model and utilized a technique comparable to that used in Trait-Treatment-Interaction (TTI) research (although in our case, one "treatment" group was the untreated control). The question was posed formally as an interactive question: did the treatment-control difference for one type of child differ significantly from the treatment-control difference for another type of child? Regression equations were run for each project, using five separate outcome measures: Stanford-Binet IQ at age 6, WISC-R (Wechsler) IQ scores in 1976, assignment to special education, retention in grade, and met school requirements ("on grade") versus failure to meet requirements (retained or assigned to special education).

The following general equation was used:*

Outcome = PRETESTIQ + SEX + MED + SIBS + FP + PC + PREPC + SEXPC + MEDPC + SIBSPC + FPPC, where:

Outcome = one of the five outcome variables listed above. School competence variables: not in special education, not retained = 1.

SEX = dummy variable for sex (female = 1)

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*RACE (Black = 1, White = 0) and RACEPC were included only in the school outcome equations for the Levenstein and Miller projects because all other projects were entirely (or almost entirely) black. School outcome equations reported here were done by OLS. Identical LOGIT equations were run with similar results except where otherwise noted (see Feinberg, 1979).

PRETESTIQ = pretest IQ scores, Stanford-Binet, except for Levenstein which is Peabody Picture Vocabulary Test.

MED = mother's education in grades completed at entry into research project.

SIBS = number of siblings at child's entry into research project.

FP = father presence dummy variable at entry into research project (father present = 1).

PC = program versus control dummy variable (program = 1).

PREPC = PRETESTIQ x PC crossproduct vector.

SEXPC = SEX x PC crossproduct vector.

MEDPC = MED x PC crossproduct vector.

SIBSPC = SIBS x PC crossproduct vector.

FPPC = FP x PC crossproduct vector.

The equation simultaneously looked at two-way interactions between program status and five background characteristics: pretest IQ, mother's education, sex, number of siblings, and father presence.

Two specific questions were asked for each outcome: (a) across projects, was there any evidence that early education programs had differential effects with respect to the whole set of background characteristics included in the equation? and (b) was there any evidence of interaction between a specific program and a specific background characteristic?

Analysis of the first question consisted of three steps. First, for each project, an $\underline{F}$ value was computed for the change in R^2 attributable to the set of interactions:

$$F = \frac{(R^2_{Y\cdot MI} - R^2_{Y\cdot M})k_I}{(1-R^2_{Y\cdot MI})/(n-k-1)} \text{, where}$$

$R^2_{Y\cdot M} = R^2$ for equation with main effects only.

$R^2_{Y\cdot MI} = R^2$ for equation with main effects and interactions.

k = total number of independent variables including crossproduct terms.

k_I = number of crossproduct vectors (interactions).

Second, the F was converted to a z value from the normal distribution. Third, the z's were squared and summed across all projects and compared to a chi-square statistic with degrees of freedom equal to the number of projects. The p-value for this chi-square gives the significance level for the effect of the interaction terms as a set when pooled across projects (Mosteller & Bush, 1954). This procedure is nondirectional -- it investigates only the contribution of the interactions to explained variance of the dependent variable and not the direction of this effect.

The second question was addressed by looking at the p-values of the individual crossproduct vectors for each project. A significant positive interaction for SEXPC, for example, would indicate that the program-control difference for females was greater than the program-control difference for males. Because of the large number of significance tests performed, the Bonferroni-Ryan correction for multiple comparisons was performed. This technique involves multiplying the significance level of the individual results by the number of tests performed (Ryan, 1959).

Results

The Stanford-Binet at Age 6

The Stanford-Binet scores at age 6 give an indication of the early effects* of programs on a standardized test of "intellectual ability". As shown in Table 3, with regard to the question of whether the effects of the set of interactions contribute to explained variance when results are pooled across projects, the pooled p-value was not significant (nor were the F values for change in R^2 for the individual projects).

None of the individual interaction terms were significant even before correction for multiple comparisons. Thus, there is no evidence that any of the Consortium early education programs had differential effects when the Stanford-Binet IQ score at age 6 was used as the dependent variable.

WISC-R IQ at 1976

WISC-R scores in 1976 provided the latest available indications of performance on intellectual tests. The WISC-R yields three IQ scores: a full-scale IQ, a verbal IQ, and a performance IQ. All three were used as separate dependent variables.

As shown in Table 4, there is little indication of interactions between program and the individual background variables on any of the three IQ scores, even before correction for multiple comparison. For the question of the set of interactions, the F values for change in R^2 were not significant for any project. For full-scale IQ, the only significant interaction was that between program and mother's education for the Gordon project. This interaction was also significant for both verbal and performance IQ's. More detailed analyses of the Gordon data indicated that the interaction was probably an artifact of characteristics of our sample.* No other interaction terms were significant for either the Full-scale IQ or Performance IQ. For Verbal IQ, there were two interactions which were significant before correction: Gray showed a negative interaction between pretest IQ score and program (indicating that the program-control score difference was greater for those with the lower pretest IQ scores), while Levenstein showed a negative interaction between sex and program (indicating that the program-control IQ score difference was greater for boys than for girls). As stated above, none of these interactions were significant after the Bonferroni-Ryan correction.** Results were not pooled across projects, since the WISC-R's were administered at different ages (and different numbers of years after the program), and, thus, there was no basis for pooling results.

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*Age 6 constituted immediate post-test for Gray, 1 year after program for Miller and Weikart, 2 years after for Levenstein, 2 or 3 years after for Palmer and 3 or more years after for Gordon.

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*There were only 11 control children in the equation and these 11 showed a negative correlation between mother's education and IQ score. We consider this negative correlation spurious in light of the fact that a positive correlation (usually between .2 and .4) between mother's education and IQ has been one of the most consistent finding in our study.

**For Gray PREPC, uncorrected p = .037, corrected p = .073. For Levenstein SEXPC, uncorrected p = .017, corrected p = .086. For Gordon Full, MEDPC, uncorrected p = .010, corrected p = .058. For Gordon Verbal MEDPC, uncorrected p = .020, corrected p = .121. For Gordon Performance MEDPC, uncorrected p = .019, corrected p = .113.

School Competence Variables

Results for the three school competence variables special education placement, grade retention, and the composite variable of failure to meet school requirements (special education placement or grade retention) are summarized in Table 5. For special education assignment, for all five projects the set of program-by-background interaction terms did not add significantly to the set of linear background terms (pooled $p = .120$). The increment in R^2 obtained by adding the set of interaction terms to the linear terms was statistically significant for Weikart only ($F = 3.393$, $p = .007$). Examination of Weikart's individual interaction terms shows that this effect was due to significant F values for number of siblings and father presence crossproducts, $p = .003$ and $p = .004$, respectively before correction for multiple comparisons. These results remained significant after a conservative correction factor was applied (corrected $p = .015$ for number of siblings and corrected $p = .016$ for father presence). The sign of the beta coefficient for the interaction terms indicates that apparently Weikart's preschool program was more effective for children who had more siblings and who came from father absent families.* None of the other individual interaction terms reached significance for special education assignment.

For grade retention, there were no significant findings for the set of interaction terms and the pooled set of interactions was not significant (pooled $p = .393$). Only a very few individual interaction terms in the Gray and Levenstein projects reached significance before correction for multiple comparison. For Gray, the sex-by-program interaction term was significant ($p = .023$ before correction, but $p = .115$ corrected). This result should be interpreted with caution. Although the OLS regression analysis program was able to solve the Gray equation with the five interaction terms, logit analysis was unable to solve the Gray equation because of Gray's small sample size (only 51 subjects) resulting in no cases for several combinations of the categories of the independent variables. The pattern of empty cells is such that not all interaction terms could be included in the logit analysis. Because we could not replicate the results with logit and because of the small cell sizes and empty cells, we feel less confident in the statistical validity of the level of significance for this interaction term.

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*Using logit analysis, the resultant p values were essentially identical to the OLS results reported in Table 5.

For Levenstein, the retention race interaction term was significant (uncorrected $p = .004$, corrected $p = .008$), but again logit was unable to solve the equation with all six interaction terms.* Further examination revealed that in Levenstein's sample, this result was based on only one white control child who was retained and five white program children who were not retained. The mother's education interaction term for Levenstein was significant ($p = .036$, uncorrected; $p = .216$, corrected), but was also problematic because when the equation was run without the race interaction term, the mother's education interaction term was not significant.

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* We are thus less confident in that statistical validity of this finding.

For the composite outcome, failure to meet school requirements, the set of background interaction terms did not add significantly to the linear terms for the pooled six projects or for any of the individual projects. None of the individual interaction terms were significant after correction for multiple comparisons. Only Weikart's father presence and number of siblings crossproducts reached significance before correction ($p = .018$ and $p = .015$; $p = .09$ and $p = .09$ corrected).*

Conclusions

Across a range of educational outcome measures, we found no convincing evidence that either sex, mother's education, pretest IQ score, father presence or number of siblings differentially affected the outcomes of any of our Consortium early education programs. A few interaction terms were significant before correction for multiple comparisons, but only three individual interaction terms remained significant after the Bonferroni=Ryan correction.

One possible explanation for the lack of significant findings is methodological limitations -- the relatively small sample sizes involved, choice of the outcome and independent variables, and problems of measurement in both the independent and dependent variables (e.g. restricted range). Howver, given the large number of significance tests performed, one would expect that if there indeed were differential effects, a stronger indication would have been evident despite methodological problems.

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*Because of a special interest in whether preschool programs were more effective for children with higher or lower pretest IQ scores, a separate series of equations were run with only three variables in the equation: program/control status, pretest IQ and pretest IQ-by-program interaction term. No significant interactions were found.

The research presented here gives no evidence of differential effects of program with the pretest IQ and demographic variables investigated -- i.e., the interaction of background variables with program did little to explain the variation in program outcomes. One must conclude that the interaction between child and treatment which affects program effectiveness is more complex than the interaction of selected demographic characteristics and program.

For policy makers concerned with "matching programs to children", the implications are that it does not appear that a "match" can be achieved through the categorization of children on a few standard demographic variables. Our findings provide no evidence that early education affects a specific type of low-income child more than others. The few instances of differential effects which were found were not consistent across outcome measures. Thus, although we have found that well-run early education programs benefit low-income children "on the average," we have not discovered any background characteristics which interact with the programs in such a way as to benefit some children more than others.

Table 1

Characteristics of Early Education Programs and Ages of Subjects for Each Data Set

Principal Investigator	Project Name and Location	Delivery System	Birth Year	Age at Entry	Program Length	Years of Program
Beller	The Philadelphia Project; Philadelphia, PA	Center	1959	4 yrs.	1 yr.	1963-64
Deutsch	Institute for Developmental Studies; New York, NY	Center	1958-1966	4 yrs.	5 yrs.	1963-71 (8 waves)
Gordon	Parent Education Program; Gainesville, FL	Home	1966-1967	3-24 mos.	1-3 yrs.	1966-70 (3 waves)
Gray	Early Training Project; Murfreesboro, TN	Center/ Home	1958	3.8 or 4.8 yrs.	14 or 26 mos.	1962-65
Karnes	Curriculum Comparison Study; Champaign-Urbana, IL	Center	1961-1963	4 yrs.	1-2 yrs.	1965-66 (2 waves)
Levenstein	Mother-Child Home Program; Glen Cove, Manhasset & Freeport, NY	Home	1964-1968	2 or 3 yrs.	1-2 yrs.	1967-72 (5 waves)
Miller	Experimental Variation of Head Start Curricula; Louisville, KY	Center & Home/Center	1964	4 yrs.	1 yr.	1968-69

Table 1 (Cont.)

Principal	Name	Delivery	Birth	Age at	Program	Years of
Investigator	Name and Location	System	Year	Entry	Length	Program
Palmer	Harlem Training Project; New York, NY	Center	1964	2 or 3 yrs.	1 or 2 yrs.	1966-68
Weikart	Perry Preschool Project; Ypsilanti, MI	Center/ Home	1958-1962	3 or 4 yrs.	1 or 2 yrs.	1962-67 (5 waves)
Woolman	Micro-Social Learning System; Vineland, NJ	Center	1966-1968	4 to 5 yrs.	1 to 4 yrs.	1969-73
Zigler	New Haven Follow-Through Study; New Haven, CT	Center	1962-1964	5 yrs.	4 yrs.	1967-71 (2 waves)

Table 2

Background Characteristics of the 1976 Follow-Up Sample

Project (n)	% Program	% Black	% Female	Mean Ed. of Mother	Mean SES	Mean Pretest IQ	Mean No. Siblings
Beller (126)	38.1	89.7	50.0	10.5	60.8[a]	90.9	3.1
Deutsch (97)	64.9	100.0	50.5	10.3	63.2	92.0	3.9[a]
Gordon (107)	77.6	94.4	53.3	9.9	65.1[a]	--	2.6
Gray (77)	46.8	100.0	55.8	9.2	69.5	87.7	4.0
Karnes (168)	100.0	64.9	51.2	10.1	64.0	92.3	3.4
Levenstein (186)	71.5	95.2	46.2	10.5	64.0	93.6	2.6
Miller (141)	77.3	92.2	53.9	10.7	64.0	93.2	3.2
Palmer (228)	78.9	100.0	0.0	11.2	58.2	92.2	2.4
Weikart (123)	47.2	100.0	41.5	9.4	68.2	79.0	4.0
Woolman (611)	31.8	--	43.0	--	--	--	--
Zigler (144)	54.2	89.1	44.2	11.1	--	--	3.0
Median	64.9	94.8	50.0	10.4	64.0	92.1	3.2

Note. Last four columns are based on somewhat less than total sample size for most projects. Mean education of mother is grades completed. SES is Hollingshead Index of Social Position, ranging from 11 to 77, highest number is lowest social position. Beller retrospectively collected mother's education, SES, and sibling data for 1976 follow-up sample only.

[a] Mean figure is based on less than 60% of 1976 follow-up sample.

Table 3

F Values for Interaction Terms for Binet IQ at Age Six

Project (n)	PREPC	MEDPC	SEXPC	SIBSPC	FPPC	Effect of Set of Interactions
Gordon (94)	--	1.241	0.850	-0.544	--	0.715
Gray (59)	-1.877	-0.525	2.965	0.937	-0.592	1.212
Levenstein (79)	-3.672	3.363	-1.759	1.349	-0.232	1.759
Miller (216)	--	0.336	0.011	0.132	-0.481	0.215
Palmer (253)	--	0.454	--	0.0002	-0.508	0.407
Weikart (120)	1.317	-0.632	-0.041	0.834	-3.829	1.280

Set of interactions: pooled $p = .558$ (chi-square = 4.891, df = 6)

Equation. Outcome = CONSTANT + PREIQ + RACE + MED + SEX + SIBS + FP + PREPC + RACEPC + MEDPC + SEXPC + SIBSPC + FPPC.

Legend. PC = program/control status; PREIQ = pretest IQ score; RACE = ethnic group; MED = mother's educational level; SIBS = siblings; FP = father presence. Background by treatment crossproducts: PREPC = PREIQ X PC; RACEPC = RACE X PC; MEDPC = MED X PC; SEXPC = SEX X PC; SIBSPC = SIBS X PC; FPPC = FP X PC. Dashes indicate that data for the independent variable were all constant or not available.

Note. F-values for first six columns are F-tests for regression coefficients for cross-product vectors. Negative sign indicates negative coefficient for cross-product vectors, i.e. program-control difference is greater for lower levels of mother's education, lower pretest IQ scores, father absence, fewer siblings, male gender, and white ethnic group. Final column indicates F-test for change in R^2 due to addition of set of interaction terms. No interaction terms were significant even before correcting for multiple comparisons.

Table 4

_F_ Values for Interaction Terms for WISC-R IQ

Project (n)	PREPC	MEDPC	SEXPC	SIBSPC	FPPC	Effect of Set of Interactions
			Full Scale IQ			
Gordon (69)	--	7.164**	0.207	-0.700	--	3.466
Gray (52)	-1.683	-0.004	-0.133	-0.338	-0.090	0.141
Levenstein (76)	--	-0.981	-3.299	-0.023	-0.465	1.668
Miller (122)	--	-0.082	-0.743	-0.398	-0.000	0.046
Palmer (131)	--	-0.243	--	0.514	-0.558	0.446
Weikart (110)	1.947	0.832	1.010	1.609	0.374	1.210
			Verbal IQ			
Gordon (69)	--	5.706*	0.156	-0.835	--	2.626
Gray (52)	-4.675*	0.023	-0.057	-0.091	0.065	0.174
Levenstein (76)	--	-0.003	-5.980*	-0.390	0.014	1.833
Miller (122)	--	-0.348	-0.643	-3.187	-0.037	2.018
Palmer (131)	--	-0.353	--	0.012	-0.123	0.044
Weikart (110)	2.339	0.365	1.483	1.632	0.011	1.217
			Performance IQ			
Gordon (69)	--	5.821*	0.096	-0.528	--	2.600
Gray (52)	-0.016	-0.106	-0.059	-0.381	-0.336	0.007
Levenstein (76)	--	-1.989	-0.760	0.085	-1.707	1.172
Miller (122)	--	0.001	-0.369	0.462	0.045	0.013
Palmer (131)	--	0.000	--	0.024	-0.494	0.019
Weikart (110)	0.847	0.784	0.384	1.025	0.683	0.775

Note. For equation and legend, see Table 3.

*$p < .05$ before correction for multiple comparisons.

**$p < .01$ before correction for multiple comparisons.

Table 5

F Values for Interactive Terms for School Competence

Project (n)	PREPC	RACEPC	MEDPC	SEXPC	SIBSPC	FPPC	Set of Int.
Special Education Placement							
Gordon (64)	--	--	0.372	0.336	0.792	--	0.941
Gray (55)	-2.016	--	-0.001	0.029	-0.292	2.220	0.672
Levenstein (119)	0.757	0.084	-0.122	-2.341	-0.360	-0.302	0.957
Miller (106)	--	-0.774	-0.774	0.006	0.000	0.122	0.441
Weikart (123)	0.740	--	-0.025	1.188	9.364**	-8.821**	3.393**
Set of Interactions: pooled p = .120 (chi-square = 8.743, df = 5)							
Grade Retention							
Gordon (53)	--	--	1.538	1.040	0.005	--	1.072
Gray (51)	-2.465	--	0.176	5.617*	-0.221	-0.185	1.630
Levenstein (104)	3.534	-8.821**	-4.537*	-0.102	-2.657	3.168	2.010
Miller (101)	--	0.073	-0.017	0.006	-0.084	-0.185	0.108
Palmer (219)	--	--	-0.000	--	0.250	1.061	0.428
Weikart (99)	1.932	--	0.137	0.336	0.040	-0.757	0.753
Set of Interactions: pooled p = .393 (chi-square = 6.280, df = 6)							
Failed to Meet Requirements (Special Ed or Retained)							
Gordon (59)	--	--	1.000	0.325	0.325	--	0.976
Gray (55)	-1.145	--	0.250	3.460	-0.005	-0.130	1.003
Levenstein (121)	1.822	-1.904	-0.122	-1.690	-0.757	-0.260	1.092
Miller (120)	--	-0.706	0.462	0.008	-0.004	0.102	0.312
Palmer (219)	--	--	-0.000	--	0.250	1.061	0.428
Weikart (123)	2.045	--	0.115	0.410	6.150*	-5.760*	2.547*
Set of Interactions: pooled p = .337 (chi-square = 6.834, df = 6)							

Note. For legend and equation, see Table 3.

*$p < .05$; **$p < .01$ before correction for multiple comparisons.

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